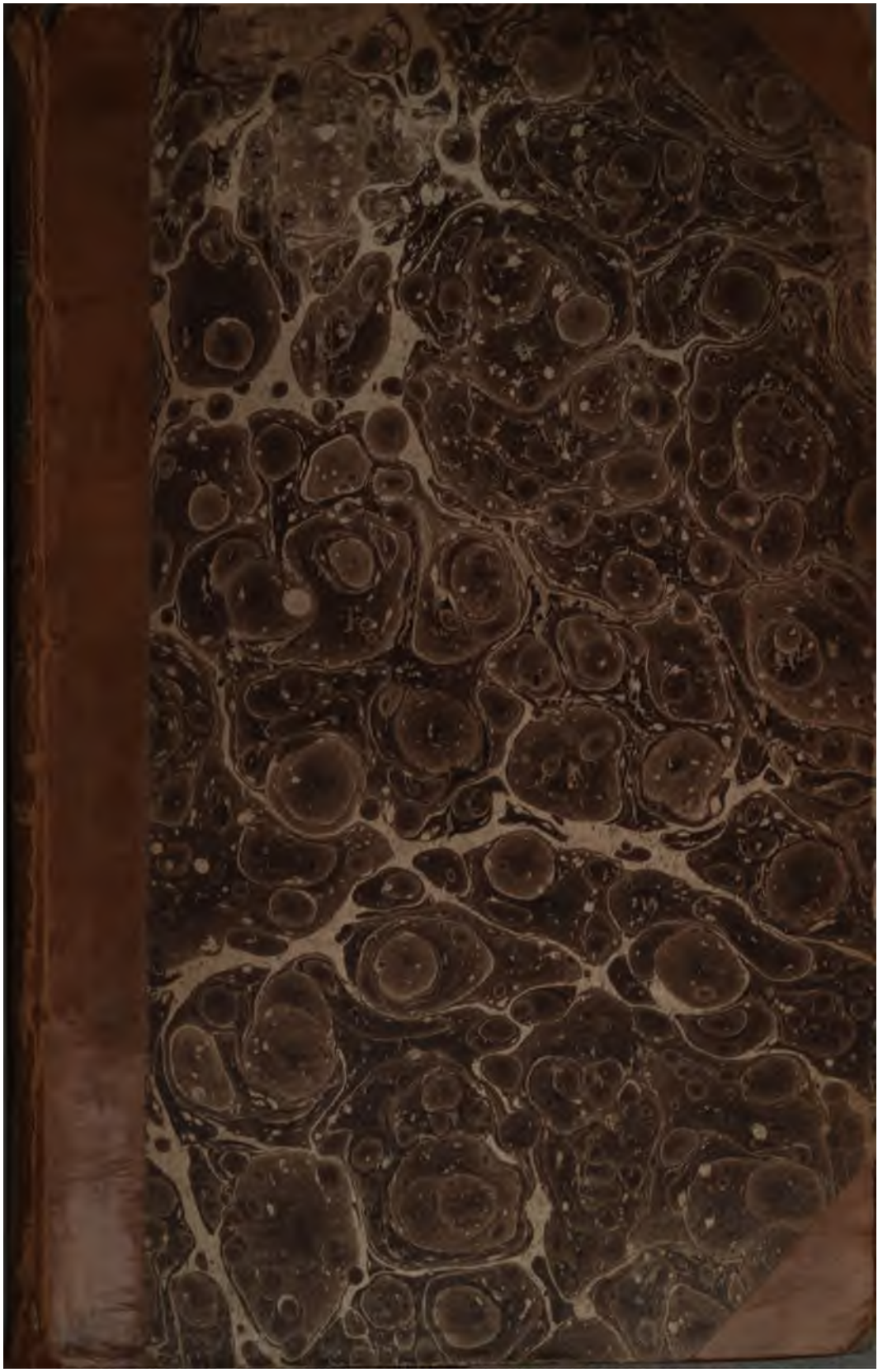

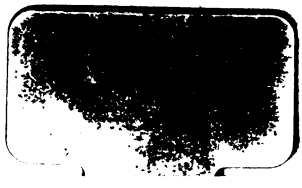

A

COMPARATIVE VIEW

OF THE

HUTTONIAN AND NEPTUNIAN

SYSTEMS OF GEOLOGY:

IN ANSWER TO THE

ILLUSTRATIONS OF THE HUTTONIAN THEORY OF THE EARTH, BY PROFESSOR PLAYFAIR.

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PREFACE.

THE preſent publication owes its origin to the "Illuſtrations of the Huttonian Theory of the Earth," by Profeſſor Playfair. In that work this theory is ſo ably ſupported, its principles are placed in ſo advantageous a point of view, the arguments which appear to favour it are ſo forcibly urged, and objections ſo ingeniouſly, and often ſucceſsfully obviated, that it has given to the diſcuſſion of this ſubject, an intereſt and form in a great meaſure new. To the Author of the preſent treatiſe, the Huttonian doctrines, whatever may be their ingenuity and novelty, appear viſionary and inconſiſtent with the phenomena of Geology; and to a defence of them ſo able, and ſo well calculated to convey a favourable impreſſion of the general ſyſtem, he has been induced by that intereſt which every

one feels in the opinions he believes to be juſt, to endeavour to reply. Although, oppoſing the Huttonian Geologiſts, he has been anxious to avoid that ſpirit of hoſtility which too frequently pervades controverſial writings: he has wiſhed to ſtate the arguments on both ſides without partiality, and to reſt the defence of the theory he ſupports on its intrin ſic worth.

It will be admitted, that there are few queſtions more calculated to intereſt the ſpeculative inquirer, or more faſcinating, from the grandeur and novelty of the objects it brings before the mind. Nor can it be ſaid to promiſe nothing but the gratification of a vain curioſity. The maxim is too well eſtabliſhed by the hiſtory of ſcience to require proof or illuſtration, that the conſequences which may reſult from any phyſical diſcovery can never be foreſeen, and that no inveſtigation can be deemed unprofitable, which may add to our knowledge of nature. A perfect Theory of the Earth, were it eſtabliſhed, would undoubtedly admit of the moſt important applications; and a ſucceſſion of

theoretical diſcuſſions may not leſs contribute to its attainment, than the accumulation of facts. Syſtems, ſays a geological writer, are in the ſciences what the paſſions are in the human mind: they may be the ſource of great errors, but they are the cauſe alſo of great exertions. Either in defending or oppoſing them, it is neceſſary to obſerve with accuracy, to compare and generaliſe; objects apparently minute, acquire an intereſt and importance; views are ſuggeſted which often lead to real acquiſitions; facts are arranged which would have remained iſolated; and relations traced which would not have been obſerved.

EDINBURGH, December 8. 1802.

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ERRATA.

Page 5.	line 24.	*For* Plutonic	*read* Vulcanic.
13.	14.	gneizs	gneiſs.
171.	25.	time	true.

INTRODUCTION.

THE object of GEOLOGY is to unfold the ſtructure of the globe,—to diſcover by what cauſes its parts have been arranged,—from what operations have originated the general ſtratification of its materials, the inequalities with which its ſurface is diverſified, and the immenſe number of different ſubſtances of which it is compoſed.

Reſearches of this kind have by ſome been deemed uſeleſs, from the ſuppoſition that their objects cannot be attained. We know not the hiſtory of the revolutions of the globe, but we find every where indications of their vaſt magnitude and antiquity. We have the moſt unexceptionable proof that its whole ſurface has been

covered with the ocean, and that every part of it has ſuffered change: mountains have been raiſed, plains levelled, iſlands ſeparated from continents, and the waters collected ſo as to leave an elevated land. We find it difficult even to conceive cauſes adequate to the production of ſuch effects; and operations ſo immenſe ſeem too remote, from any means of inveſtigation we poſſeſs, to admit of being explained.

The reply which may be given to obſervations of this kind is ſatisfactory. In any ſubject whatever, where the *media* of proof by which a propoſition is to be eſtabliſhed, are imperfectly known, and where the propoſition itſelf is remote from the more familiar objects of obſervation and reſearch, to inveſtigate it is always conſidered as impracticable; and the attempt is treated as viſionary, until it be crowned with ſucceſs. He who knows not the facts of geology, and the evidence they afford, may be diſpoſed to deride its pretenſions and diſtruſt its concluſions: but a more intimate acquaintance with the ſubject, or even a moderate knowledge of the progreſs of ſcience, will teach us to reject ſuch narrow views. Queſtions apparently not more within the reach of inveſtigation have been completely ſolved; and man who has weighed the planets, and meaſured their diſtances, may preſume to

trace the operations by which the ſurface of the globe has been arranged.

On making a nearer approach to what at a diſtant view appears ſo difficult, we find that the path we have to purſue is even traced out; that our inveſtigations are directed by facts which derive not their ſupport from theory, but are eſtabliſhed by the cleareſt evidence. It is proved that the whole ſurface of the globe has at one period been in a fluid ſtate, and that from this has originated its preſent arrangement. It is certain that this fluidity muſt have been effected by the operation either of fire or of ſome ſolvent. In our farther reſearches, therefore, we are nearly limited to the inquiry, by which of theſe means this effect has been induced; and from the facts which apparently are within the reach of diſcovery, we need not deſpair of ſolving the problem. The effects of fuſion, and of ſolution, are in general ſo very diſſimilar, or at leaſt the peculiarities of their action are ſo well marked, that from an attentive examination of the properties of minerals, and of the ſtructure and poſition of the great maſſes of the globe, we may hope to diſcover to which of theſe they owe their origin and arrangement; and if this diſcovery be eſtabliſhed, the labours of geologiſts will be confined to tracing the opera-

tion of this agent in producing the phenomena obſerved. Their explanations may at firſt be imperfect, and their inductions require to be often corrected; but ſtill ſuch inveſtigations muſt contribute to the real progreſs of the ſcience, and may at length eſtabliſh a perfect theory.

The fact which has been ſtated as the baſis of all geological inquiries,—that the ſurface of the globe has been in a fluid ſtate, is eſtabliſhed by very ample evidence. In the greater number of the ſtrata of the earth, in the moſt elevated, as well as in thoſe at the greateſt depths, ſubſtances are found in a cryſtallized ſtate; and even many of theſe ſtrata have marks of cryſtallization in their entire ſtructure. Cryſtallization is the arrangement of the particles of a body in a regular determinate form; and it neceſſarily implies a previous ſtate of fluidity which would allow theſe particles to arrange themſelves in poſitions neceſſary to produce theſe forms. Many of the moſt ſolid ſtrata contain in their ſubſtance remains or impreſſions of animals and vegetables; and it is obvious, that to admit of the introduction of ſuch ſubſtances, they muſt at one time have been, if not in a perfectly fluid, at leaſt in a ſoft or yielding ſtate. Laſtly, the general diſpoſition of the materials of the globe, ſo far as has been explored, muſt have ariſen

from fluidity, as this only could have arranged them in beds or ſtrata parallel to each other, and preſerving that paralleliſm to a great extent. Theſe appearances are not partial: they extend to the whole ſurface of the earth, and indubitably prove its former fluidity.

It is obvious that fluidity can be ſuppoſed to be produced only in two modes. Either the ſolid matter muſt have been fuſed by the action of heat, or it muſt have been diſſolved in ſome fluid. Theſe are accordingly the primary propoſitions of the different geological theories that have been advanced. Some have ſuppoſed the ſurface of the globe to have been ſoftened or melted by the operation of fire; others have aſcribed the fluidity it muſt once have had, to ſolution; and as there is no fluid in ſufficient abundance to have acted the part of a ſolvent of a quantity of matter ſo large, except water, *aqueous ſolution* has been adopted as the cauſe of the original fluidity of minerals, in oppoſition to *igneous fuſion*.

Theſe oppoſite ſyſtems have been diſtinguiſhed by the fanciful appellations of the Plutonic and the Neptunian. It would be a ſuperfluous taſk to examine the different modifications of them that have been propoſed. Of thoſe which aſcribe the formation of minerals to fuſion, it

will probably be admitted that that advanced by Dr. Hutton is ſuperior to any in the extent and connection of its principles, and in the adaptation of them to the explanation of the phenomena. It will therefore be ſufficient to compare the Huttonian theory with the modern Neptunian ſyſtem.

PART I.

Statement of the Huttonian and Neptunian Theories.

THE ſyſtem which Dr. Hutton has traced of the formation of the ſtrata of the globe, is calculated to captivate the imagination by its grandeur, and by the appearances which it exhibits of regularity and deſign. It preſents a ſeries of changes, each following from, or connected with the other, and ſo nicely adapted, as to be apparently capable of being carried on for an unlimited time. It is claimed as its peculiar excellence, "That it aſcribes to the phe-
"nomena of geology an order ſimilar to that
"which exiſts in the provinces of nature with
"which we are beſt acquainted; that it pro-
"duces ſeas and continents, not by accident,
"but by the operation of regular and uniform
"cauſes; that it makes the decay of one part
"ſubſervient to the reſtoration of another; and

" gives ſtability to the whole, not by perpetuat-
" ing individuals, but by reproducing them in
" ſucceſſion *."

Dr. Hutton conceives that in this globe there is a ſyſtem of decay and renovation, and that the proceſſes by which theſe are effected have an uniform relation to each other. The ſolid matter of the earth is of ſuch a nature that it muſt be waſted by the powers which continually operate upon it. The hardeſt rocks are worn down by air and water: cauſes which, however ſlowly they may operate, are conſtant in their action, and which therefore, in indefinite time, muſt be equal to the production of the greateſt effect. From the figure of the ſurface of the earth, the decayed materials muſt be carried towards the ocean, and ultimately depoſited in its bed. Though this tranſportation may be impeded by local cauſes, or may in general be extremely ſlow, yet from the declivity of the land it muſt neceſſarily take place, and may therefore be admitted as an uniformly operating cauſe.

It is farther aſſumed, that at great depths in the mineral regions an immenſe heat is conſtantly preſent, and that this heat operates in the fuſion and conſolidation of the ſubſtances depoſited in theſe regions. It is to the action of this ſubterraneous fire that the formation of all our ſtrata

* Illuſtrations of the Huttonian Theory, p. 129.

is attributed. They conſiſt of the wrecks of a former world, which have been more or leſs perfectly fuſed by this agent, and by ſubſequent cooling have been conſolidated.

The ſubterraneous fire to which Dr. Hutton aſcribes thoſe effects, he conceives to operate at the ſame time under a particular modifying circumſtance, which gives a peculiar character to his ſyſtem, and frees it from many objections which were unanſwerable in former hypotheſes of a ſimilar kind. This circumſtance is compreſſion. The ſubterraneous fire being placed at immenſe depths, the ſubſtances on which it operates muſt be under a vaſt preſſure. This will prevent their volatilization in whole or in part; and from this circumſtance it is poſſible to explain appearances in minerals, and qualities they poſſeſs, which would otherwiſe appear inconſiſtent with the ſuppoſition of their being formed by fire.

The laſt part of the Huttonian doctrine is that which regards the elevation of the ſtrata which have been thus formed; and this likewiſe is ſuppoſed to be the effect of ſubterraneous heat. Its expanſive power muſt occaſionally be exerted with very great force; and it is inferred that this muſt have been the cauſe which elevated theſe ſtrata, becauſe no other can be aſſigned adequate to the production of this effect.

Such is the general outline of the HUTTONIAN THEORY. It is not an attempt to explain the firſt formation of the globe, nor does it proceed on the ſuppoſition, that its preſent is its original ſtate. It reſts on the principle, that, according to eſtabliſhed laws, or from neceſſary changes, operations are conſtantly carrying on, by which the portion of ſolid matter elevated at the ſurface is worn down, and by which the decay is rendered ſubſervient to the formation of new ſtrata, which will be elevated in their turn. Our world is formed from the decay of one which preceded it, and is now furniſhing the materials from which another is to be raiſed; and this great natural operation is without limitation as to time. On the one hand, we find in the ſyſtem itſelf no veſtige of a beginning; on the other, no proſpect of an end.

From this theory are explained the appearances which our ſtrata preſent, in their ſtructure and their poſition. With reſpect to ſtructure, we find conſiderable variety; ſome, as granite, are compoſed chiefly of ſubſtances in a cryſtallized ſtate, and theſe are ſuppoſed to have been completely fuſed; others, as ſandſtone or chalk, are heterogeneous in their texture, or imperfectly conſolidated; and theſe are ſuppoſed to have been only in a ſoftened ſtate; and between theſe exiſt many intermediate degrees. With reſpect to

poſition, ſome are horizontal, ſome inclined, and others vertical, irregular, or abrupt; and ſuch appearances muſt have ariſen from the operation of that power by which they were raiſed. In their firſt formation at the bottom of the ocean, their arrangement muſt have been horizontal; but in their elevation, by an expanſive power acting from beneath, their continuity muſt have been broken, and every variety of poſition produced.

Leſs magnificent in its pretenſions than the ſyſtem, the outlines of which we have now traced, is the NEPTUNIAN THEORY, or that which ſuppoſes the ſtrata of the earth to have had an aqueous origin. From the appearances which foſſils preſent, it is inferred that they cannot have been formed by fuſion; and as ſolution is the only other mode in which we can conceive ſoftneſs or fluidity to have been effected, it is ſuppoſed to have been the agent by which the matter at the ſurface of the globe was conſolidated. With this ſimple concluſion, it might be preferable, in the preſent ſtate of geological knowledge, to reſt ſatisfied, ſince it is proceeding perhaps as far as obſervation can guide us. To go farther is to wander in the regions of hypotheſis and fancy; and though the opinion thus deduced might not have the impoſing exterior

of a complete ſyſtem, it would be more likely to poſſeſs the ſtability of truth.

Such ſtrict and cautious induction, however, is not calculated to ſatisfy the inquiries of the theoriſt. It is perhaps neceſſary to complete the ſyſtem; not merely to reſt ſatisfied with the proof that minerals have been formed by ſolution, but to attempt to ſhow how this ſolution, and the ſubſequent conſolidation from it, have been effected. At leaſt, in the preſent diſcuſſion, this is doing juſtice to the Huttonian Theory, as placing both on equal ground.

It may be obſerved, however, in proceeding to the ſtatement of that modification of the Neptunian ſyſtem which is generally received, that its author, Werner, has not indulged in hypotheſis, but has approached as nearly to an induction of facts as the ſubject admits. From the poſitions and connections of the ſtrata, he finds reaſon to conclude that they have been formed by water. From their relative ſituations it is evident that ſome have been formed prior to others, and this priority of formation he endeavours to trace and generaliſe. And, laſtly, from their ſtructure it is apparent that ſome have been chemical, others mechanical depoſits, diſtinctions which of courſe ought to enter into the ſyſtem.

The firſt principle, then, of the Wernerian theory is, that the materials of which our ſtrata

conſiſt were at one period diſſolved or ſuſpended in water, and that from this fluid they had ſucceſſively conſolidated in various combinations, partly by cryſtallization, and partly by mechanical depoſition. Granite being the rock which compoſes the moſt elevated part of the globe, and which likewiſe forms the baſis on which the greater number of the ſtrata reſt, is ſuppoſed to have been firſt formed, the different parts of which it conſiſts, felſpar, quartz, and mica, having concreted by a cryſtallization nearly ſimultaneous. This was accompanied and followed by the ſimilar conſolidation of the other primitive ſtrata, gneizs, micaceous ſhiſtus, argillaceous ſhiſtus, porphyry, quartz, &c.

Theſe rocks compoſe the chief elevations of the globe. They are never found to contain any organic remains, and of courſe their formation muſt have been prior to the exiſtence of the vegetable and animal kingdoms.

From the period of the formation of thoſe ſtrata, it is ſuppoſed that the water covering the ſurface began to diminiſh in height, by retiring gradually into cavities in the internal parts of the earth. During this period, other precipitations, chiefly chemical, but in a few caſes partly mechanical, continued to take place. Theſe formed what Werner terms the intermediate ſtrata, or ſtrata of tranſition, of which ſome va-

rieties of limeſtone, ſhiſtus, and trap, are the principal. They are incumbent on the primary, and ſometimes, though rarely, contain petrifactions; a proof, however, that organization, or at leaſt the exiſtence of marine animals, had commenced with their formation.

The diminution of the water ſtill continued to proceed, and by the mechanical action of its maſs on the ſtrata formed, it occaſioned in them a partial diſintegration. The materials from this ſource, together with the remaining part of the matter originally diſſolved, by their precipitation and conſolidation formed what are named the ſecondary ſtrata, or the ſtratified rocks, ſandſtone, limeſtone, gypſum, pudding-ſtone, ſome varieties of trap, and various others. Theſe are of a height much inferior to the former. From the predominance of mechanical depoſition, they are arranged generally in horizontal beds, and they are abundant in organic remains; a proof of their formation being poſterior to the full developement of the animal and vegetable kingdoms.

Of theſe different formations the whole ſurface of the globe is formed, at leaſt with the trivial additions of the products of volcanic fire, and the alluvial beds of ſand, clay, and ſoil, ariſing from the waſte of the ſtrata by the waters which run over them.

During the conſolidation of theſe ſtrata, rents happened in them, by which cavities of very various dimenſions were formed. Into theſe the water, holding various matters in ſolution, gained acceſs, and hence the formation of mineral veins.

Such are the ſyſtems which we have to compare. In the examination of them, we may conſider them under the following diviſions: Firſt, we may inveſtigate the probability of their principles, and the objections to which theſe, *a priori*, are liable: And, ſecondly, we may inquire how far they derive ſupport from the ſtructure and arrangement of the ſurface of the globe, and from the appearances which minerals actually exhibit.

PART II.

Of the probability of the Firſt Principles of the Huttonian and Neptunian Theories.

THE firſt of the ſeries of ſuppoſitions which compoſe the Huttonian theory is, that the ſtrata which form the ſurface of the globe, are, from the nature of their conſtitution, and the conſtant operation of the agents to which they are expoſed, liable to decay; that by being worn down and tranſported to the ſea, they furniſh materials for new ſtrata to be formed and afterwards elevated; and that from the decay of a former world our ſtrata have originated.

To part of theſe ſuppoſitions fewer valid objections perhaps can be made than to any of the other principles of the Huttonian ſyſtem. It ſeems to be a neceſſary effect ariſing from the nature of the ſolid materials at the ſurface of the globe, that they muſt be waſted by the powers which act upon them; and many facts

have been ſtated, which eſtabliſh ſuch a diſintegration. The immenſe beds of ſand and gravel which are found even in elevated ſituations, afford in particular a clear proof of this kind, ſince ſuch ſubſtances muſt evidently have been formed from the waſte of ſolid rocks, and the attrition of their fragments.

Facts have, however, likewiſe been ſtated in oppoſition to this principle. Rocks of baſaltes, and of many kinds of granite, it is obſerved, ſuffer ſcarcely any decay, as is evident from the ſharpneſs of their angles. And from the teſtimony of hiſtory, we have it in our power to aſcertain, that in many places rocks of this kind occupy the ſame ſituation which they did two thouſand years back. Theſe, and ſimilar facts, however, are to be regarded as exceptions to the general law; or rather, they ſerve to prove, that in ſuch rocks the diſintegration is extremely ſlow. It ſeems ſcarcely poſſible to deny but that it muſt take place to a certain extent; and as the operations ſuppoſed in the Huttonian ſyſtem are unlimited as to time, it would be in vain to conteſt the principle aſſumed.

It has alſo been queſtioned, whether the materials worn down, and conveyed by the rivers, are carried into the depths of the ſea. It has been alleged, that the greater part of the matter thus conveyed is thrown by the returning tide

upon the ſhore, and adds to the extent of the land. In many caſes, from local ſituation, this is undoubtedly true; but Profeſſor Playfair has perhaps ſucceſsfully ſhown that theſe are exceptions, and that from the declivity of the ſhore, the ſolid matter brought by the rivers muſt in general be carried forward, and ſpread over the bottom of the ocean. Were this not the caſe, indeed, the increaſe of the land, from the accumulation of materials on the ſhore, might be expected to be more rapid than it actually is.

Yet though this much be admitted, it is far from eſtabliſhing the concluſions which Dr. Hutton has deduced, that this diſintegration is part of a ſeries of changes going on in conſtant ſucceſſion, or that it makes part of a ſyſtem in which a habitable world is always preparing from one exiſting, the place of which it is to ſupply. If the diſintegration be ſo ſlow as is admitted; if, as Dr. Hutton himſelf obſerves, the deſcription which Polybius has given of the Pontus Euxinus, with the two oppoſite Boſphori, the Mæotis, the Propontis, and the port of Byzantium, are as applicable to the preſent ſtate of things as they were at the writing of that hiſtory; if the iſthmus of Corinth is apparently the ſame at preſent as it had been two or three thouſand years ago; if Scylla and Charybdis remain now as they had been in ancient times,

rocks hazardous for coaſting veſſels; if the port of Syracuſe, with the iſland which forms the greater and leſſer, and the fountain of Arethuſa, the water of which the ancients divided from the ſea with a wall, do not ſeem to be altered; and if on the coaſt of Egypt we find the rock on which was formerly built the famous tower of Pharos; and at the eaſtern extremity of the port Eunoſte, the ſea-bath, cut in the ſolid rock on the ſhore, to all appearance the ſame at this day as they were in ancient times;—if ſuch be the extreme ſlowneſs of the diſintegration, the reflection is obvious, that, admitting it, a duration will be allowed to the world infinitely beyond our conception, and adequate to any purpoſe which we can conceive it deſigned to ſerve; and there is at leaſt no neceſſity pointed out for ſuppoſing an arrangement by which it is to be perpetuated or reſtored.

Neither are the facts concluſive which are ſtated by Dr. Hutton and Mr. Playfair, to prove that all our ſtrata have originated from the waſte of a former world, for they are equally well accounted for by the Wernerian ſyſtem. It is ſtated, that many rocks are found which contain fragments of others, or which are connected with collections of gravel, looſe or conſolidated. Such fragments and gravel neceſſarily ſuppoſe the exiſtence of former ſtrata, from the waſte of

which they had originated. It is alſo obſerved, that in many of the moſt extenſive ſtrata of the earth, remains or impreſſions of organic ſubſtances are found, both animal and vegetable, and of courſe theſe muſt have exiſted prior to the formation of ſuch ſtrata.

Theſe facts are conſidered by the Huttonian geologiſt as ſufficient proof of the exiſtence of a habitable world, from the decay of which ours has been formed. They are however equally well accounted for by the Neptuniſt, without admitting ſuch a ſuppoſition. It is ſuppoſed that the exiſtence of marine animals commenced after the cryſtallization of the great primary ſtrata; and that after that period too the waters of the ocean began to diminiſh in height, ſo as to leave elevated land, on which vegetation commenced. The retreat of the ocean continued to be gradual for many ages, and during this time the ſecondary ſtrata were formed. It is obvious, therefore, that the fragments of rock, the ſand and gravel which theſe often contain, or with which they are aſſociated, or which even in many caſes compoſe the greateſt part of their maſs, might originate from the diſintegration of the primary ſtrata above the level of the ſea; a diſintegration to which, in this early period of their conſolidation, they might even be more liable than they now are. And the origin

of the remains of marine animals, and even of vegetables, found in the ſecondary rocks, it is obvious are equally well accounted for on this theory, ſince the exiſtence of theſe may have begun previous to the formation of theſe ſtrata. The facts, therefore, do not prove the hypotheſis of Dr. Hutton, ſince on a different hypotheſis they are explained with equal facility.

It has been affirmed, however, that the ſame appearances of ſand and gravel, and of marine impreſſions, are occaſionally to be met with in the primitive ſtrata, and that of courſe the Wernerian explanation is defective; for marine animals are not ſuppoſed to have exiſted at their formation; and it is obvious that the preſence of ſand and gravel are true indications of ſtrata having exiſted before them.

But it is aſſerted, on the other hand, by Neptunian geologiſts, that ſuch appearances are not to be met with in ſtrata truly primitive, but that when they do occur in ſtrata not of the ſecondary claſs, it is in thoſe of the intermediate kind, or what Werner terms the rocks of tranſition. Theſe, it will be recollected, are ſuppoſed to be poſterior in their formation to the primary, but prior to the ſecondary ſtrata, and to have been formed at that period when the exiſtence of marine animals, or at leaſt of ſome ſpecies of them, had commenced; and of courſe they may occaſionally

be found with impreſſions or remains of theſe beings. This ſuppoſition is liable to no difficulties, and ſeems to follow juſtly from the facts. Since certain rocks, having peculiar characters, and compoſing the moſt elevated parts of the globe, are found deſtitute of organic remains, while in others they are abundant, does not this afford a preſumption, that the former had been produced prior to the period when theſe beings began to exiſt? And if rocks are found intermediate in their characters between theſe, connected principally with the primary, but in general leſs elevated, and ſometimes, though rarely, containing veſtiges of ſea animals, is it not reaſonable to believe that theſe have been intermediate in their formation, and that at leaſt the few ſpecies of thoſe animals, whoſe remains are found in them, had begun to exiſt at the time they were formed?

It will be found that ſuch a ſuppoſition accords much better with theſe phenomena, and affords a more ſatisfactory ſolution of them than the hypotheſis by which they are explained in the Huttonian ſyſtem. According to that ſyſtem, all the ſtrata, both thoſe termed primitive as well as thoſe named ſecondary, have been formed from materials depoſited at the bottom of the ocean from the wrecks of a former world. They ought therefore all equally to contain or-

ganic remains and impreſſions; and it remains to be accounted for why theſe ſhould be entirely wanting in many of the ſtrata, as in gneiſs, or micaceous ſhiſtus, while they are preſent in others. There is only one ſuppoſition by which this can be attempted. It is, that the former ſtrata have been in more complete fuſion than the latter, and that thus by the more intenſe heat, theſe remains, or the impreſſions of them, have been deſtroyed. But the explanation is contradicted by the appearances of theſe ſtrata, the marks of fuſion being frequently as complete in theſe which contain ſuch remains, as in thoſe which do not. Thus there are many limeſtones and marbles containing ſhells, in which the ſparry ſtructure is as perfect as it is in the primary limeſtone, and in which are even diſtributed veins of cryſtallized carbonate of lime; and in the tranſitive limeſtone, the parts which contain no marine impreſſions have no marks of more perfect fuſion than theſe parts in which they are preſent, nor indeed is there any difference between them. In like manner, the primitive ſhiſtus, which has no marks of foreign bodies, has no appearances of more complete fuſion than the ſecondary ſhiſtus, in which vegetable impreſſions are abundant. The ſuppoſition, therefore, of the Huttonian geologiſt to account for the abſence of organic remains from theſe

ſtrata, is not merely hypothetical, but inconſiſtent with the phenomena, while that of the Neptuniſt is ſo probable, that it ſeems to follow as a corollary from the facts.

The Huttonian explanation does not derive more ſupport from the other general fact that has been ſtated in proof of the production of all our ſtrata from the wrecks of a former world, —the aſſociation of breccias, of ſand or gravel with the primary rocks. Several inſtances of this kind, ſtated by Profeſſor Playfair are extremely doubtful. But granting that there are ſuch appearances which cannot be diſputed, they are eaſily explained. It is obvious that on the gradual retreat of the ſea, which happened after the formation of the primary rocks, the moſt elevated would firſt be left bare; and whenever this happened, the maſs of water would begin to act upon this dry land, and occaſion in it diſintegration. It is even reaſonable to believe, though it is not neceſſary to make the ſuppoſition, that this diſintegration would proceed more rapidly immediately after their conſolidation than when they had been more fully hardened by time. Whether this were the caſe or not, fragments of the elevated rocks muſt have been worn down into ſand, gravel, &c. and by the direction of currents, banks of theſe might be depoſited among the exiſting or forming

ſtrata, and might either, according to circumſtances, be left in a looſe ſtate, or be conſolidated by the depoſition of the matter ſtill diſſolved or ſuſpended in the fluid. There is no difficulty therefore attending the fact, that ſuch collections are found in the tranſitive, or even in the truly primitive ſtrata, nor does it eſtabliſh the concluſion that ſuch ſtrata had originated from the waſte of older continents.

There is even a particular phenomenon in this part of geology, very general in its occurrence, which admits of a natural and ſatisfactory explanation from the Neptunian theory, while it requires the moſt extravagant ſuppoſition when conſidered according to the principles of the Huttonian ſyſtem. The appearance is that of a bed of breccia incumbent on the primary ſtrata, and covered by one or other of the ſecondary ſtrata. Over the vertical ſtrata, for example, of the primary ſhiſtus, there is frequently a bed of breccia, compoſed of fragments of rocks conſolidated by ſome cement; and over this bed is a ſtratum of ſandſtone or limeſtone. According to the Neptunian theory, the explanation of this is very eaſy. Of theſe the ſhiſtus, from its poſition, it is evident muſt have been firſt formed: other primary rocks, however, had been formed at a ſtill earlier period, and in poſitions more elevated. As the ſea retreated

theſe would be left bare, and by their diſintegration would afford the fragments which, depoſited on the lower ſhiſtus, formed the breccia; and this again, before the retreat of the ſea was completed, ſo as to leave it expoſed, might be covered by a depoſit of ſandſtone or limeſtone.

The explanation, according to the Huttonian hypotheſis, involves a ſuppoſition ſo extraordinary as to furniſh a ſingular contraſt with that of the Neptunian. It is ſuppoſed * that the ſhiſtus had been formed in beds nearly horizontal, and that by an expanſive power exerted from beneath, theſe had been elevated to the ſurface, and placed in a vertical poſition. In this ſituation, the bed of gravel from which the breccia is formed, had been depoſited on the ſummit of the vertical ſhiſtus. To admit of the formation of the horizonal ſtrata of ſandſtone, it is further ſuppoſed, that the ſhiſtus, with this ſuperincumbent breccia, had again ſunk in the ocean, and remained depreſſed for ages, till the materials of the ſandſtone were depoſited on it. Theſe materials are ſuppoſed to have been then conſolidated by the central fire operating on them, even with the intervention of the deep ſtrata of ſhiſtus on which they are incumbent; and laſtly, we are told that the whole, when thus prepared, were again elevated by a new exertion of

* Illuſtrations, &c. p. 52

heat. It may ſurely be affirmed, without farther reaſoning, that ſuppoſitions ſo extravagant and improbable can never be real interpretations of the operations of nature.

We may on the whole conclude, that though the exiſtence of organic impreſſions and remains may prove that the ſtrata containing them have been formed poſterior to the exiſtence of the vegetable and animal kingdoms; and though the preſence of breccia, ſand and gravel in the ſtrata may likewiſe prove that other ſtrata had exiſted, from the waſte of which they had been produced, yet neither of theſe facts eſtabliſhes the concluſion, that our ſtrata have originated from the decay of a former world; and the explanations of the Huttonian ſyſtem on this ſubject are even leſs probable and ſatisfactory than thoſe given by the Neptunian theory. The general propoſition may therefore be admitted, that the ſtrata of the earth are liable to waſte, and that the materials are carried forward to the ſea, without proving from any appearances that this makes part of a ſeries of changes, or is a ſtep in the ſucceſſion of worlds, or that on this planet a habitable world has exiſted prior to the preſent, and from the waſte of which this has originated.

The ſecond principle aſſumed in the Huttonian theory, is, that the materials which are collected at the bottom of the ocean are at great depths expoſed to the action of an intenſe heat, under a ſtrong preſſure, by which they are fuſed and conſolidated, ſo as to be capable of forming new ſtrata. This principle may be conſidered under different aſpects; and the diſcuſſion of it is important, ſince it is capable of affording a *direct demonſtration of the falſity of the Huttonian hypotheſis.*

The difficulties which attend the opinion may firſt be ſtated. How is this immenſe heat produced? If from combuſtion, the only probable ſource, whence is the combuſtible matter derived by which it is excited? Whence is the oxygen ſupplied, by which that combuſtion muſt be kept up? And how can it be applied to materials collected and depoſited from water, while the operation of that water is at the ſame time excluded? Theſe are difficulties which may be urged againſt this hypotheſis, and of which it will not be an eaſy taſk to give a ſatisfactory ſolution.

The intenſity of the heat required to produce that fuſion whence our ſtrata could have originated is beyond what it is poſſible, perhaps for the imagination clearly to conceive. This is evident from the immenſe extent of theſe ſtrata.

The higheſt mountains of the globe run in extenſive chains, and theſe, from their connection, muſt neceſſarily have been formed at one time. It is not leſs evident, from the difficult fuſibility of the ſubſtances either compoſing their entire ſtructure, or contained in them. This may be illuſtrated by one or two examples.

Carbonate of lime, of which all the calcareous cryſtals, marbles, limeſtone and chalk, conſiſt, cannot be fuſed by any heat which we can command, or at leaſt can be fuſed only in the moſt minute quantity. Lavoiſier was unable to melt a particle of it by the intenſe heat excited by a burning mirror; and Sauſſure, by the much more powerful heat excited by the flame of the blow-pipe, urged by a ſtream of oxygen gas, was able only to fuſe a particle of it, ſo ſmall that it required the aid of the microſcope to diſcover it. What then, ſays Mr. Kirwan, muſt have been the heat neceſſary to melt whole mountains of this matter? "Judging from all "we at preſent know of heat, ſuch a high de- "gree could only be produced by the pureſt "air acting on an enormous quantity of com- "buſtible matter. Now Ehrman obſerved, that "the combuſtion of 280 cubic inches of air "acting on charcoal was not able to effect the "fuſion of one grain of Carrara marble; from "whence it is apparent, that all the air in the

" atmoſphere, nor in ten atmoſpheres, would " not melt a ſingle mountain of this ſubſtance " of any extent, even if there were a ſufficient " quantity of inflammable matter for it to act " upon *."

It has been attempted to leſſen, if not to obviate the force of this objection. Profeſſor Playfair has obſerved, that " this reaſoning is not " applicable to Dr. Hutton's hypotheſis of ſub- " terraneous heat, becauſe it is grounded on ex- " periments where that very ſeparation of the " volatile and fixed parts takes place, which is " excluded in that hypotheſis. When lime- " ſtone or marble is expoſed to ſuch a heat as " is here mentioned, or even to heat of a de- " gree vaſtly inferior, the carbonic gas is ex- " pelled, and the body is reduced to pure lime. " The Carrara marble may require a heat of " 6300 of Wedgewood to melt it in the open " air, where the carbonic acid gas eſcapes from " it; but under ſuch a preſſure as would retain " this gas, it cannot be inferred that it might " not melt with the heat of a glaſs-houſe fur- " nace †."

This argument, that carbonate of lime may be ſo much more fuſible than pure lime, and that therefore the ſubterraneous heat by which

* Geological Eſſays, p. 453.
† Illuſtrations, p. 184.

it is ſuppoſed to have been fuſed, may be very moderate, ſince it acted under a compreſſion by which the carbonic acid would be retained, is ſuppoſed to be confirmed by an analogical fact. Dr. Black had remarked, that pure barytes is much leſs fuſible than carbonate of barytes, ſince, when carbonate of barytes is expoſed to an intenſe heat, it firſt fuſes or vitrifies; it then begins to part with its carbonic acid, and as it does ſo, it returns to the ſolid ſtate. It is ſuppoſed, by analogy, that the caſe may be the ſame with carbonate of lime, that when ſtrong compreſſion is applied, it may be fuſed by a much lower heat than when the carbonic acid is allowed to eſcape. And ſuch a compreſſion is ſuppoſed, in the Huttonian theory, to have been applied in the mineral regions, where the immenſe maſſes of carbonate of lime have been formed.

The fact, it may be obſerved, on which this argument is founded, is doubtful. In the mode of obtaining the barytic earth, by decompoſing carbonate of barytes by heat, the barytes is not obtained pure; and if the experiment be made, as it uſually is, in an earthen veſſel, as the decompoſition proceeds, the barytes acts upon the earth of the veſſel, and forms with it a ſpecies of frit. The ſubſtance obtained in this way is both much leſs fuſible and leſs ſoluble than the

pure earth obtained by decompoſing the nitrate of barytes, according to the proceſs of Vanquelin. This earth is indeed ſo eaſily fuſible, that the fact cannot be admitted, that barytes, when pure, is more infuſible than when combined with carbonic acid. Preciſely the reverſe is the caſe.

But were it true, the analogy thence extended to lime is not juſt. Carbonate of barytes, it is ſaid, fuſes before it parts with its carbonic acid; but carbonate of lime does not. No analogy, therefore, exiſts between them in the increaſed fuſibility ariſing from the combination of carbonic acid with theſe earths; or there are no juſt grounds for ſuppoſing that lime, like barytes, is rendered more fuſible by combination with that acid; and therefore there is no foundation for this argument of Profeſſor Playfair. And even if there were, ſtill the heat requiſite to fuſe carbonate of lime muſt be intenſe. In order to decompoſe it, and expel the carbonic acid, a white heat is requiſite, and ſtill at this temperature the calcareous carbonate is not fuſed; and its fuſion would probably require a much greater heat than this. When we conſider, therefore, the great extent of the ſtrata of calcareous matter, it is evident, that admitting the above analogical argument to be juſt, which it is not, ſtill an immenſe heat muſt have been neceſſary for

their conſolidation, according to the Huttonian hypotheſis, a heat with which that of a glaſs-houſe furnace cannot be compared.

The original objection may alſo be ſtated with equal force, with reſpect to other foſſils in which there is not the ſame mode of eluding it. Quartz, for example, according to the experiments of Lavoiſier, and other chemiſts, is nearly as infuſible as lime. It not only remains unmelted in any heat a furnace can raiſe, but in the heat excited by a burning mirror. Now there are entire mountains of quartz; it is found in large veins in many rocks, and in others, as in granite, it exiſts cryſtallized as a conſtituent part. Theſe, therefore, muſt have had an intenſe heat applied to them for their fuſion; and no modification, from the preſence of preſſure, or any other circumſtance that could have any effect in promoting the fuſion of the quartz, can be imagined. The heat of a glaſs-houſe furnace, ſays Mr. Playfair, truſting in the impoſſibility of aſcertaining the preciſe fuſibility of carbonate of lime, may have been ſufficient for its fuſion. But there is not the ſame uncertainty with regard to quartz. It is calculated by Sauſſure, that it requires for its fuſion a temperature equal to 4043 of Wedgewood's pyrometer. Glaſs, at a medium, requires only 30 of the ſame ſcale. From the compariſon of theſe, therefore, we may judge of the intenſi-

ty of the heat which would be neceſſary to fuſe the ſtrata of the earth, and how far the ſtandard of compariſon which Profeſſor Playfair points out is juſt.

The force of this argument, we may eſtimate from the care with which the anſwer to the objection, from the infuſibility of carbonate of lime, is repeatedly ſtated, and fully urged. In enumerating the difficulties attending the Huttonian hypotheſis, from the intenſe heat which muſt have been required to fuſe certain ſubſtances, the two examples of carbonate of lime and quartz are the moſt obvious, and have been generally ſtated together. The defender of that hypotheſis, diſcovers an analogical argument, by which it is ſuppoſed, that the force of the objection, from the infuſibility of one of theſe ſubſtances, the calcareous carbonate, may be obviated; and this is brought forward, ſtrongly ſtated, and repeated under various forms. But the other example, the quartz, is kept out of view. No notice is taken of its infuſibility having been brought forward equally with that of the calcareous matter, as demonſtrating the improbability of this principle of the Huttonian doctrine. Is not this a tacit admiſſion of the force of the objection? Where there appears a poſſibility of anſwering it, even an imperfect analogical argument, founded on a miſtake in point of fact, is

not diſdained. When, in another inſtance, therefore, it is not attempted to anſwer it, is not this to admit, that no ſatisfactory reply can be given? that, of courſe, the objection, from the extreme infuſibility of foſſil ſubſtances, of which carbonate of lime and quartz are given as the moſt ſtriking examples, remains in full force? and that the anxiety diſplayed in obviating it, in one inſtance where it ſeemed poſſible to do ſo, is a proof that the objection is not, in the eſtimation even of the defenders of this ſyſtem, trivial, but one which they would wiſh, if poſſible, to remove?

So far, therefore, the improbability of this principle of the Huttonian theory, that foſſils have been fuſed by a ſubterraneous heat, muſt appear evident from the difficulties which attend it. Whether we conſider the extent of the ſtrata thus ſuppoſed to have been fuſed, or the extreme infuſibility of the matter of which they are compoſed, the heat requiſite muſt exceed, in intenſity, not only any that we know, but even any that the imagination can conceive; and for the production of ſuch a heat, no adequate cauſe can be aſſigned.

But this argument, however forcible, may ſtill be placed in a ſtronger point of view. It is the peculiar feature, and as has been ſtated by its author, the excellence of the Huttonian

theory, that the operations it ſuppoſes are intimately connected, and are carried on in a ſucceſſion to which no limits are placed. It does not account merely for the appearances which the earth at preſent exhibits, but unfolds a ſyſtem in which the deſtruction and formation of ſucceſſive habitable worlds are explained. In every part of it, therefore, proviſion muſt be made, not merely for the production of an effect, but for the reproduction of that effect an indefinite number of times. Indulge the defender of this hypotheſis with the aſſumption, that there exiſted in the bowels of the earth a quantity of combuſtible matter ſufficient to have produced a heat capable of having fuſed the preſent ſtrata, and that a ſufficient quantity of air was ſupplied to ſupport its burning, even this will avail him little. Theſe ſtrata are ſuppoſed to have originated from the decay of others which exiſted before them of a ſimilar kind, and which had been formed in the ſame manner; and they are to be ſucceeded by new ſtrata likewiſe fuſed and elevated by heat. Nothing can be more incontrovertible, than that any accumulation of combuſtible matter, or any collection of air which can be imagined, would not, from the very principles of the ſyſtem, be ſufficient to maintain ſuch operations. It is a propoſition indeed ſelf-evident, that a limited quantity could

not ſerve to ſupport operations, not only immenſe in their extent, but unlimited in their ſucceſſion.

On the other hand, Dr. Hutton cannot ſhow that there is any proceſs carried on at the ſurface of the earth ſufficiently extenſive to ſupply combuſtible matter for theſe operations. He ſeems to imagine that vegetation is capable of ſerving this purpoſe. "Let us ſuppoſe," ſays he, "the ſubterraneous fire ſupplied with its "combuſtible materials from this ſource, the "vegetable bodies growing upon the ſurface of "the land: Here is a ſource provided for the "ſupplying of mineral fire; a ſource which is "inexhauſtible or unlimited, unleſs we are to "circumſcribe it with regard to time and the "neceſſary ingredients *."

It is true that vegetation is the great ſource of combuſtible matter at the ſurface of the earth; it ſerves as a counterpoiſe to the various kinds of oxygenation, decompoſes their products, diſengaging the oxygen, and accumulating in the matter of plants carbon and hydrogen. Theſe two proceſſes appear deſigned in the economy of nature as antagoniſts to each other, oxygenation conſuming the combuſtible matter which vegetation produces, and vegetation de-oxydating the products of oxygenation. But

* Theory of the Earth, vol. I. p. 243.

there is no reaſon to believe that the one general proceſs exceeds the other in the ultimate effect. It might perhaps be urged as an objection to the Huttonian theory of the formation of coal, that it derives that ſubſtance from the exceſs of combuſtible matter produced at the ſurface; for it would require proof that there is any ſuch exceſs, or at leaſt ſuch an exceſs as to continue for an unlimited time. But at any rate it can never be pretended that from this ſource can be derived that immenſe quantity of combuſtible matter neceſſary not only to form the coal of the ſucceeding world, but to ſupport that intenſe combuſtion which is required to fuſe and conſolidate its earthy ſtrata.

It even admits of demonſtration that no operation carrying on at the ſurface of the globe can furniſh an uninterrupted ſupply of combuſtible matter. In the formation of the ſtrata, the Huttonian hypotheſis labours under no difficulty with reſpect to providing materials, becauſe the ſame matter which at one period is fuſed and elevated, is again worn down. But there is no ſuch ſucceſſion of combuſtible matter: in combuſtion it is neceſſarily rendered unfit to ſupport the ſame proceſs until it be de-oxydated; and there is no mean pointed out, or even none which can be imagined, by which this is effected.

Were even this difficulty removed, it recurs in preciſely the ſame force with reſpect to the ſupply of oxygen, which is juſt as neceſſary as combuſtible matter to combuſtion. No accumulation of oxygen can ſupport combuſtion for an unlimited time; and no ſource can be imagined whence a ſucceſſive ſupply could be derived.

Profeſſor Playfair ſeems to be ſufficiently aware of the force of theſe objections, and of the impoſſibility of ſuppoſing the ſubterraneous heat to be excited and preſerved by any ſpecies of combuſtion; and this, as limiting the diſcuſſion, is an important conceſſion. According to all the appearances, he obſerves, from which the exiſtence of a central heat has been inferred, "it "is of a nature ſo far different from ordinary "fire, that it may require no circulation of air, "and no ſupply of materials, to ſupport it. It "is not accompanied with inflammation or "combuſtion, the great preſſure preventing "any ſeparation of parts in the ſubſtances on "which it acts, and the abſence of that elaſtic "fluid, without which heat ſeems to have no "power to decompoſe bodies, even the moſt "combuſtible, contributing to the unalterable "nature of all the ſubſtances in the mineral "regions *." Again, "In a region where the

* Illuſtrations, &c. p. 93.

" action of heat was accompanied with ſuch " compreſſion as is here ſuppoſed, there could " be no fire properly ſo called, and no combuſ- " tion. This is admitted by Dr. Hutton; and " it is therefore a fallacious argument which is " brought againſt his theory, from the impoſſi- " bility of fire being maintained in the bowels " of the earth. This impoſſibility is preciſely " what he ſuppoſes, and yet Mr. Kirwan's ar- " guments are directed not againſt the exiſtence " of heat in the interior of the earth, but a- " gainſt the exiſtence of burning and inflam- " mation *."

Profeſſor Playfair having thus admitted, in the moſt expreſs terms, that the ſubterranean heat, ſuppoſed to be the cauſe of ſuch important effects in the Huttonian hypotheſis, cannot ariſe from any ſpecies of combuſtion, proceeds to explain the mode in which he conceives it may be excited. His obſervations on this ſubject muſt be quoted at full length. " It is not " FIRE in the uſual ſenſe of the word, but HEAT, " which is required for that purpoſe; and there " is nothing *chimerical* in ſuppoſing, that nature " has the means of producing heat, even in a " very great degree, without the aſſiſtance of " fuel or of vital air. Friction is a ſource of " heat, unlimited, for what we know, in its ex-

* Illuſtrations, p. 182.

" tent, and ſo perhaps are other operations, both " chemical and mechanical; nor are either " combuſtible ſubſtances, or vital air, concerned " in the heat thus produced. So alſo the heat of " the ſun's rays in the focus of a burning glaſs, the " moſt intenſe that is known, is independent of " the ſubſtances juſt mentioned; and, though " that heat certainly could not calcine a metal, " nor even burn a piece of wood, without " oxygenous gas, it would doubtleſs produce as " high a temperature in the abſence as in the " preſence of that gas.

" It is true, that it is not by the ſolar rays " that ſubterraneous heat is produced; but " ſtill, from this inſtance, we ſee, that there is " no incongruity in ſuppoſing the production " of heat to be independent of combuſtible bo- " dies, and of vital air. We are indeed, in " all caſes, ſtrangers to the origin of heat: " philoſophers diſpute, at this moment, con- " cerning the ſource of that which is produced " by burning; and much more are they at a " loſs to determine, what upholds the light and " heat of the great luminary, which animates " all nature by its influence. If we would " form any opinion on this ſubject, we ſhall do " well to attend to the ſuggeſtions of that great " philoſopher, who was hardly leſs diſtinguiſhed " from others by his doubts and conjectures,

" than by his moſt rigorous and profound in-
" veſtigations. 'May not great, denſe, and fix-
" ed bodies, when heated beyond a certain de-
" gree, emit light ſo copiouſly, as, by the emiſ-
" ſion and reaction of its light, and the reflec-
" tions and refractions of its rays within its
" pores, to grow ſtill hotter, till it comes to a
" certain period of heat, ſuch as is that of the
" ſun? And, are not the ſun and fixed ſtars
" great earths, vehemently hot, whoſe heat is
" conſerved by the greatneſs of the bodies, and
" the mutual action and reaction between them,
" and the light which they emit * ?'

" Some recent experiments ſeem to make the
" ſuggeſtions in this query applicable to an o-
" paque body like the earth, as well as to lumi-
" nous bodies, ſuch as the ſun and fixed ſtars.
" The radiation of heat, where there is no light,
" was firſt rendered probable by the experi-
" ments of M. Pictet of Geneva †; and the
" only objections to which the concluſions from
" thoſe experiments ſeemed liable, are removed
" by the late very important diſcoveries of Dr.
" Herſchel ‡. From theſe it appears that heat
" is capable of refraction and reflection, as well

* Newton's Optics, *ubi ſupra.*

† Eſſai ſur le Feu.

‡ Phil. Tranſ. 1800. p. 84.

" as light, ſo that it is not abſurd to ſuppoſe,
" that *the heat of great, denſe, and fixed bodies,*
" *may be conſerved by the greatneſs of the bodies,*
" *and the mutual action and reaction between them*
" *and the heat which they emit* *."

We may admire the ingenuity of theſe obſervations, in which an argument, obvious and incontrovertible, is ſo enveloped and diſguiſed, that it requires ſome diſcuſſion to place it in its clear light. To what purpoſe are the various ſources of heat, enumerated in this reaſoning? To prove that it may exiſt, or be produced independent of burning. This will be readily granted: But the reaſoning can prove nothing farther; it can never warrant the concluſion, that heat may be aſcribed to the operation of an unknown cauſe. If it ariſe not from burning, it muſt be concluded that it is produced by ſome of its other known cauſes, by friction, the ſun's rays, ſome ſpecies of chemical action, or any other; and if it can be ſhown, that the ſuppoſed ſubterranean heat cannot originate from any of theſe cauſes, the objection to the hypotheſis which admits it is as ſtrong and irreſiſtible, as if combuſtion were the ſole origin of heat, and it were proved that it could not be the effect of that proceſs. Now, it can be ſatisfactorily ſhown, that any of the known

* Illuſtrations, &c. p. 186, 187, 188.

cauſes of heat, are as incapable of producing it in the interior parts of the globe, to that extent which muſt be ſuppoſed in the Huttonian theory, as combuſtion, which, even by its defenders, is confeſſed to be inadequate to that purpoſe. Hence the general concluſion againſt the ſuppoſition of the exiſtence of ſuch a heat will be rendered firm and incontrovertible.

Beſides, combuſtion, friction, and the ſolar rays, are particularly pointed out as ſources of heat. The firſt muſt be, and is indeed relinquiſhed, as the ſource of the heat ſuppoſed to exiſt in the mineral regions; and the laſt is likewiſe confeſſed to be inapplicable. Friction will not ſurely be ſuppoſed to be the ſource; for what cauſe can be aſſigned to excite and preſerve a degree of friction capable of producing ſuch an effect? All theſe cauſes ſeem, indeed, to be given up, and are apparently introduced only to pave the way for another hypotheſis, that "the "heat of great, denſe, and fixed bodies, may be "conſerved by the greatneſs of the bodies, and "the mutual action and reaction between them "and the heat which they emit;" and that to this cauſe may be owing the heat in the central regions.

This hypotheſis preſents merely an indiſtinct idea to the mind, and to be refuted requires only to be analyſed.

The diſtinguiſhing and characteriſtic property of caloric, that from which by far the greater part of its effects ariſes, is its elaſticity, or tendency to exiſt every where in a ſtate of equilibrium. In conſequence of this tendency, it cannot poſſibly be accumulated, and preſerved in that accumulated ſtate, in any body whatever. If a maſs of matter be heated to a high temperature, it immediately begins to part with caloric to the bodies around it, and the ſeparation is proved to take place in two modes; part of it is thrown off in right lines, or by radiation; and part by a ſlower communication, through the medium of the matter immediately in contact with it. According to the nature of that matter, this communication will be more or leſs ſlow, but it will always take place with a certain celerity; and even if the body be completely inſulated, or placed in a vacuum, it has been proved, by the moſt accurate experiments, that caloric continues to paſs from it, probably chiefly by radiation, but ſtill ſo as to reduce its temperature.

From theſe facts, which are rigorouſly eſtabliſhed, we are enabled to decide upon this ſubject, and to determine how far the hypotheſis, that the heat of great and denſe bodies may be preſerved by the reaction between them and the heat which they emit, is true.

If we conceive a number of large maſſes of matter at certain temperatures, placed at immenſe diſtances from each other, and either iſolated, or connected only by a very ſubtle medium, ſtill a communication of caloric muſt take place, more or leſs ſlowly, from theſe bodies to each other. A quantity will be thrown off from all of them by radiation; and if any medium exiſts between them, another portion will be given off by communication. In a certain ſenſe, this caloric will be preſerved amongſt them; it will be returned from one to another; and this mutual communication will continue till (if no foreign local cauſe diſturb it) a common temperature is eſtabliſhed among them. But if by ſaying that great and denſe bodies can preſerve their temperature by the mutual reaction between them and the heat they emit, it be meant that any of theſe bodies can, for an *unlimited* time, and without any foreign ſource of heat, preſerve itſelf at a temperature ſuperior to that of the others, we may affirm, in oppoſition even to the authority of Newton, that the concluſion is not juſt, unleſs indeed we ſuppoſe that in ſuch caſes caloric is endowed with peculiar modes of action unknown to us. The defenceleſs Huttonian may ſhelter himſelf under the diſtinction of the ancients, of heat into celeſtial and terreſtrial, and he may confer on either what properties will beſt

accord with his hypotheſis; but if we are to follow the more ſober rules of philoſophiſing, and judge of the operation of a phyſical agent from what we know of its powers, it is obvious that in a ſyſtem of bodies, whatever may be their greatneſs or denſity, heat cannot be accumulated in one or more of them for an indefinite time. It is evident, therefore, that conſidering the conjecture of Newton even under this point of view, it cannot be rendered ſubſervient to the purpoſe to which the learned Profeſſor would apply it.

But the total inſufficiency of this hypotheſis is ſtill more apparent when we regard the diſtribution of caloric, not over a ſyſtem of bodies placed at diſtances from each other, but merely over the different parts of one maſs,—the point of view in which it muſt be regarded in this diſcuſſion with regard to ſubterraneous heat. It is ſimply to be repeated, that it is an eſſential property of caloric to diffuſe itſelf over matter with which it is in contact, till an equilibrium or uniformity of temperature is produced,—a tendency which cannot be interrupted or reſiſted. Let us ſuppoſe the central parts of the earth to be heated to any point; in conſequence of this property, part of that heat muſt be diffuſed towards the ſurface in every direction; and the more denſe the earth is, the more rapid

will this diffuſion be; for although the celerity of the propagation of caloric through bodies is not preciſely according to their denſities, it is more nearly in that ratio than in any other. No heat, therefore, in the centre of the earth could be kept accumulated there; its intenſity muſt continue uniformly to diminiſh; and as the Huttonian ſyſtem ſuppoſes a ſucceſſion of worlds formed by this ſubterraneous heat, of which part ſhall always be ſolid and habitable, while another part is fluid, and in a ſtate of preparation to be raiſed, it is evident, that from the ſuppoſition made of great and denſe bodies preſerving their heat, the local temperature neceſſary for theſe operations cannot be explained. The heat producing it muſt come at length to be equally diſtributed over the whole maſs, and either render it entirely fluid, or be incapable of fuſing any part.

To this view of the ſubject, therefore, the preſervation of a central heat in a maſs of matter, which is the only one that has any relation to the preſent queſtion, the hypotheſis of Newton does not apply. That philoſopher might conceive that a great body heated, and placed at an immenſe diſtance from every other, might from its greatneſs preſerve its heat for a very long period. Or it might be ſuppoſed that a certain portion of caloric might be preſerved in

a ſyſtem of bodies either opaque or luminous, by the mutual radiation and abſorption of it among them. But neither of theſe ſuppoſitions, were they admitted, contribute in the ſmalleſt degree to ſolve the problem, how an intenſe heat could be preſerved and accumulated in the central parts of a maſs of matter ſuch as our earth. The hypotheſis, that great bodies conſerve their heat by their greatneſs, and the mutual action between them and the heat they emit, or the eſtabliſhed fact, that caloric emanates from bodies by radiation, are here of no avail: they have not in fact the moſt remote relation to the queſtion; and we may admire the ingenuity by which they are apparently connected with it. While caloric continues to be regulated by the laws according to which we know it to operate, it muſt, if accumulated in one part of a maſs of matter, diffuſe itſelf over the whole; and neither the greatneſs of the body, nor any other property with which this power is endowed, can effectually arreſt or counteract this diſtribution.

It thus appears that no adequate cauſe can be aſſigned for the production and continuance of that immenſe heat in the bowels of the earth, which is aſſumed as a firſt principle in the Huttonian ſyſtem, and that every hypotheſis on this ſubject labours under inſuperable difficulties.

We may now advance a ſtep farther, and ſhow by direct *demonſtration*, independent of the arguments that have hitherto been urged, that *this principle of the theory is falſe*. It is ſuppoſed that an intenſe heat always exiſts in the central parts of the globe, has exiſted, and muſt continue active, while this world is regulated by its preſent laws. Let this be admitted, waving every objection as to the mode in which this heat is produced or preſerved; let it be granted that there is a ſupply of combuſtible matter and of oxygen ſufficient for that purpoſe, or that great and denſe bodies by their greatneſs conſerve their heat; or let any other ſuppoſition for the production and continuance of this heat be received which the Huttonian chooſes to make; it is an invariable and eſſential property of heat to diffuſe itſelf over ſpace till an equilibrium of temperature is eſtabliſhed; and where there is any ſolid matter as the medium of diffuſion, its diſtribution is more rapid. If an intenſe heat has always exiſted at the central parts of the globe, this heat muſt diffuſe itſelf towards the circumference, and the diffuſion of it muſt continue till the whole arrive at a common temperature. The arrangement, therefore, contrived in the Huttonian ſyſtem for the ſucceſſive renewal of the habitable part of the globe, and repairing the waſte to which it is ſubjected,

is inherently defective. It is always becoming leſs fit to produce its effects, as the heat at the centre muſt always be diminiſhing; and it muſt come at length to be ſubverted by the temperature being rendered uniform over the whole. The peculiar excellence, therefore, to which the Huttonian ſyſtem lays claim, that of pointing out the means of renovation proportioned to the waſte, and which did it poſſeſs, would exalt it over every other, does not belong to it. In many parts of the economy of nature we perceive a ſeries of cauſes and effects capable of producing each other in ſucceſſion for an unlimited time, and which, while the ſyſtem is governed by its preſent laws, may be ſaid to be without any mark of a beginning, or any indication of an end. Water is raiſed by evaporation, forms clouds, deſcends again on the earth in rain, and thus a perpetual circulation of it is produced. Oxygen is conſumed by animals, and carbonic acid formed; while, by vegetables, carbonic acid is abſorbed, and oxygen evolved; and theſe counteract each other, preſerve the purity of the air at nearly an uniform ſtandard, and may be conceived to do ſo for any length of time. The deſtruction of the elevated parts of the globe, the conſolidation of the materials thus waſted, and their elevation in new ſtrata, is repreſented in the Huttonian ſyſtem, as an operation of a ſi-

milar kind; and the view which it thus holds out was indeed regarded by its author as its peculiar excellence. It may be admitted to be one conformable to the general economy of nature, and, in part, it might appear to be juſt, ſince the materials which are worn down are thoſe which are again elevated, and again liable to waſte. But in the agency by which part of this ſeries of operations is ſuppoſed to be performed, the ſyſtem fails. No proviſion is made for the continuance of the ſubterranean heat, by which the matter waſted is to be conſolidated and elevated; and, even allowing its exiſtence, from the nature of this power, it is demonſtrated, that it cannot be preſerved for an unlimited time, in that active and concentrated ſtate, which is neceſſary to perform theſe operations, but that it has a tendency to diffuſe itſelf, till it become every where equal; a tendency which no power can counteract, and which is utterly ſubverſive of what it is deſigned to perform. The ſyſtem has within itſelf a principle of decay; its operations muſt, of neceſſity, have an end, and are incapable of producing that indefinite ſucceſſion of worlds, ſuppoſed by the Huttonian geologiſt, and inſeparably aſſociated with the principles of his theory.

It is perhaps unneceſſary to urge this diſcuſſion farther. Yet there is another point of

view, under which this ſubject may be conſidered. Heat, it is ſufficiently known, is propagated through denſe bodies with conſiderable celerity. If a central fire, therefore, of the greateſt intenſity, exiſt, the heat muſt be propagated through the ſubſtance of the earth towards its ſurface; and this propagation ought to be ſuch, that, even in that period of time of which we have authentic records, its effects ought to have been apparent. Yet we have no reaſon to believe that there is any change in the medium temperature of the globe. The climate of particular countries may be altered, from cultivation, or other local circumſtances, but no important general alteration appears to have taken place; if it had, its effects muſt have been conſpicuous, by ſymptoms too well marked not to indicate their cauſe. Nay, no change of this kind appears to have happened for a much longer period than that which man has aſcertained. It may be affirmed, that the temperature which at preſent prevails, is that neceſſary for vegetation, animal life, and, in general, for all the operations of nature; nor could a habitable world like ours, have exiſted with a medium temperature many degrees inferior to that which now prevails. The heat at the ſurface, therefore, muſt have always been nearly the ſame; and, though a central heat has been ex-

iſting, according to the Huttonian theory, for that immenſe ſucceſſion of time, during which our world, and others preceding it, have exiſted, there has been no propagation of it through the ſubſtance of the earth. If we can rely on any deduction whatever from the knowledge we poſſeſs, we may reſt aſſured, that a ſyſtem involving ſuch a ſuppoſition is falſe; nor would it be eaſy even to imagine any proceſs of reaſoning by which its falſity could be more clearly demonſtrated.

If we compare the Huttonian with the Neptunian theory, it will appear evident, that, according to the one, the preſent arrangement of the ſurface of the earth admits not of any longer duration, than it does according to the other; for the principle of renovation, ſuppoſed in the former, muſt have its power extinguiſhed, before the waſte, which it is deſigned to repair, is complete. The tendency to the deſtruction of this globe, as an habitable world, ariſes from the diſintegration which the ſtrata ſuffer; but this diſintegration is acknowledged to be extremely ſlow. "We have mountains "in this country," ſays Dr. Hutton, "and thoſe "not made of more durable materials than "what are common to the earth, which are not "ſenſibly diminiſhed in their height with a "thouſand years. The proof of this are the Ro-

" man roads made over ſome of thoſe hills. I " have ſeen theſe roads as diſtinct as if only " made a few years, with ſuperficial pits beſide " them, from whence had been dug the gravel, " or materials of which they had been form- " ed*." If, in ſo long a period, the diſintegration is ſo inconſiderable as not to be perceptible, what muſt be required to level theſe mountains with the ſea? Millions of years would not ſuffice. If the compariſon be made, of the degree of celerity of the propagation of caloric, and the quantity of matter it has to penetrate in paſſing from the centre to the ſurface of the globe, and that of the celerity of the diſintegration of the exiſting ſtrata, as eſtabliſhed by theſe facts, it will be evident, that, after ſtating the former as low as poſſible, it muſt arrive at its termination; or, in other words, the temperature muſt become uniform in a ſhorter period than that which would be neceſſary to complete the diſintegration of the elevated parts of the preſent land.

It is ſcarcely neceſſary to place the abſurdities following from this principle in any other point of view; yet one more may be added, if poſſible more glaring than the others. The materials from the waſte of the preſent world are ſuppoſed

* Theory of the Earth, vol. ii. p. 140.

to be depoſited in the bed of the ocean, and to be there conſolidated by the central heat. This heat, therefore, has been capable of propagating itſelf through the immenſe maſs of ſolid matter interpoſed between the centre of the earth and the bottom of the ſea, and in a degree ſo intenſe as to conſolidate the looſe matter thus depoſited. Yet beyond this it has never been able to advance, even in that vaſt period of time in which ſucceſſive worlds have been forming: In other words, this heat has been diffuſed through ſome thouſand miles, to the moſt intenſe degree, but is there arreſted, and cannot extend itſelf a few miles farther; a circumſtance worthy to be regarded as an interpoſition of a ſuperior power in favour of man. It is thus that the verſatile Huttonian ſuſpends the laws of nature at his pleaſure, or finds them always pliant and accommodating to the principles of his hypotheſis.

From the whole of this reaſoning, it appears, that under whatever aſpect we conſider this part of the Huttonian theory, it not only labours under inſuperable difficulties, but is proved to be falſe. Whether we ſuppoſe a vaſt accumulation of heat, or of matter, capable, by combuſtion, or any other mode of producing it; or whether we ſuppoſe a ſucceſſive ſupply of heat or of matter capable of affording it, ſtill the ſup-

ply cannot be inexhauſtible, and, of courſe, cannot ſupport the operations it is ſuppoſed to perform, for an indefinite time. And, were it even adequate to this, ſuch is the nature of the agent employed, that it neceſſarily muſt prove deſtructive of the ſyſtem it is deſigned to repair. There is an accumulation of proof, which prejudice itſelf, we ſhould be tempted to believe, could not reſiſt, and which is more than ſufficient to eſtabliſh the concluſion, that whatever praiſe may be due to the Huttonian ſyſtem as a ſplendid hypotheſis, it has no claim to the more exalted rank of a juſt theory.

In this diſcuſſion on the exiſtence of a central heat, it has not been thought neceſſary to notice the argument for it, drawn from the phenomena of volcanoes; it is ſo obvious that theſe ariſe, not from any ſource of this kind, but from fire excited in the volcanic mountain. Yet, as this is an argument on which ſome ſtreſs is laid, it may not be improper to ſhow its fallacy by a few facts.

1ſt, The heat of the matter erupted from volcanoes, is not ſuch as it muſt have been, were it derived from that fire which the Huttonian geologiſt ſuppoſes to exiſt in the centre of the globe. This heat is ſuppoſed to be ſufficient to fuſe granite, and of courſe quartz, which, according to the experiments of Sauſſure, requires a tem-

perature equal to 4043 of Wedgewood's ſcale. But many facts prove, that the heat communicated by volcanic fire, to the matter it throws out, ſeldom, if ever, equals 120 of the ſame ſcale. This is evident from ſhorls, and various other foſſils, fuſible at 100, or 110, being found unaltered in the lava thrown out,—a proof that they had never been fuſed; and lava itſelf is fuſed or vitrified at a temperature below 40. Such a heat, therefore, can bear no compariſon with that ſuppoſed by the Huttonian geologiſt to exiſt in the ſubterranean regions. It is of courſe, a proof, that the lava erupted had not been derived from that ſource.

2*d*, The products of volcanoes are totally unlike thoſe which are ſuppoſed in the Huttonian theory to be formed and thrown out from the central regions. The latter conſiſt of granite, porphyry, and trap, ſubſtances never rejected by volcanoes. On the other hand, ſulphur is an abundant production of volcanic fires, while it is never preſent in the unſtratified rocks; a proof of itſelf deciſive that the matter ejected from volcanoes is not derived from the ſame ſource with that which is ſuppoſed to give riſe to the products of the central regions. The ſtony matter thrown from volcanoes appears indeed, from many facts, to be merely the rocks and foſſils

of the country, either fuſed or partially altered by the volcanic fire.

Laſtly, The extinction of volcanoes ſufficiently proves that they ariſe merely from the burning, or mutual chemical action, of a quantity of matter locally accumulated, and ſpent after a certain period. If they were connected with the central regions, no ſuch extinction ſhould take place.

The evidence from theſe facts is deciſive in proving that the volcanic mountain is the ſeat of the operations the volcano performs; and the phenomena, therefore, to which theſe give riſe, are no proof whatever of a central fire.

The force of ſome of the difficulties which have been ſtated in the courſe of this diſcuſſion, and the impoſſibility of obviating them, ſeems to have been perceived by Dr. Hutton himſelf. In anſwer to the objections of Mr. Kirwan, he makes the following obſervations, from which, however he attempts to diſguiſe it, this is ſufficiently evident. "I give myſelf little or no "trouble about that fire, (the fire neceſſary to "fuſe minerals) or take no charge with regard "to the procuring of that power, as I have not "founded my theory on the *ſuppoſition* of ſub-"terraneous fire, however that fire properly fol-"lows as a concluſion from thoſe appearances on "which the theory is founded. My theory is "founded upon the general appearances of mine-

" ral bodies, and upon this, that mineral bodies
" muſt neceſſarily have been in a ſtate of fuſion.
" I do not pretend to prove demonſtratively
" that they had been even hot; however, that
" concluſion alſo naturally follows from their
" having been in fuſion. It is ſufficient for
" me to demonſtrate, that theſe bodies muſt
" have been more or leſs in a ſtate of ſoft-
" neſs and fluidity without any ſpecies of ſolu-
" tion. I do not ſay that this fluidity had been
" without heat, but if that had been the caſe,
" it would have anſwered equally well the pur-
" poſe of my theory, ſo far as this went to ex-
" plain the conſolidation of ſtrata or mineral
" bodies, which I ſtill repeat muſt have been by
" ſimple fluidity, and not by any ſpecies of ſo-
" lution, or any other ſolvent than that univer-
" ſal one which permeates all bodies, and which
" makes them fluid.

" Our author, (Mr. Kirwan), has juſtly
" remarked the difficulty of fire burning be-
" low the earth and ſea. It is not my purpoſe
" here to endeavour to remove thoſe difficulties,
" which perhaps only exiſt in thoſe ſuppoſitions
" which are made on this occaſion; my purpoſe
" is to ſhow that he had no immediate concern
" with that queſtion in diſcuſſing the ſubject of
" the conſolidation which we actually find in
" the ſtrata of the earth, unleſs my theory with

" regard to the igneous origin of ſtony ſubſtances
" had proceeded upon the ſuppoſition of a ſub-
" terraneous fire. It is ſurely one thing to em-
" ploy fire and heat to melt mineral bodies, in
" ſuppoſing this to be the cauſe of their conſo-
" lidation, and another thing to acknowledge
" fire or heat as having been exerted upon mi-
" neral bodies, when it is clearly proved from
" actual appearances, that theſe bodies had been
" in a melted ſtate or that of ſimple fluidity.
" Theſe are diſtinctions which would be thrown
" away upon the vulgar, but to a man of ſcience,
" who analyſes arguments, and reaſons ſtrictly
" from effect to cauſe, this is, I believe, the
" proper way of coming at the truth *."

The obſervations of Profeſſor Playfair are to the ſame purpoſe: They admit in effect the difficulties attending the ſuppoſition of a central heat by which minerals have been fuſed, and endeavour to leſſen theſe difficulties, on the ground that mineral ſubſtances are proved from their appearance to have already undergone ſuch an operation. " We are not entitled, according
" to any rules of philoſophical inveſtigation, to
" reject a principle to which we are fairly led
" by an induction from facts, merely becauſe
" we cannot give a ſatisfactory explanation of it,

* Theory of the Earth, vol. I. p. 237.

" It would be a very unſound view of phyſical " ſcience which would induce one to deny the " principle of gravitation, though he cannot " explain it, or even though the admiſſion of it " reduces him to great metaphyſical difficulties. " If indeed a downright abſurdity, or inconſiſ- " tency with known and eſtabliſhed facts, be " involved in any principle, it ought not to be " admitted, however it may ſeem calculated to " explain other appearances. If, for inſtance, " Dr. Hutton held that combuſtion was carried " on in a region where there was no vital air, " we ſhould have ſaid that he admitted an ab- " ſurdity, and that a theory founded on ſuch " poſtulata was worſe than chimerical. But if " the only thing imputable to him is, that, being " led by induction to admit the fuſion of mine- " ral ſubſtances in the bowels of the earth, he " has aſſumed the exiſtence of ſuch heat as was " ſufficient for this fuſion, though he is unable " to aſſign the cauſe of it, I believe it will be " found that his ſyſtem only ſhares in an imper- " fection which is common to all phyſical theories, " and which the utmoſt improvement of ſcience " will never completely remove*."

Theſe obſervations are ſo far juſt, that the merits of any geological theory muſt reſt on its

* Illuſtrations, &c. p. 189.

according with the appearances of minerals. If it fully agrees with theſe appearances, the cauſe, thus eſtabliſhed by induction, ought to be admitted, though it may be liable to difficulties as to its mode of production or operation. But in the application of theſe obſervations to the Huttonian hypotheſis there ſeems to be a miſtake, and the reaſoning under which its defenders ſhelter themſelves as a laſt reſource, is in ſeveral reſpects incorrect.

It will be recollected, in the firſt place, that the term *proof* uſed in this reaſoning, is not to be underſtood in the ſenſe which ſtrictly belongs to it, but in a more looſe ſignification. When Dr. Hutton ſays that he has *proved* minerals to have been formed by fuſion, he cannot pretend that his proof is of that kind from which corollaries may be ſtrictly deduced, as from a mathematical demonſtration. The proof can, from the nature of the ſubject, amount to no more than a high probability, and in eſtabliſhing it, therefore, all the circumſtances are to be kept in view, and allowed their due weight. If difficulties attend the concluſion, that minerals have been formed by ſubterraneous heat, theſe muſt enter into the calculation of probabilities from which the concluſion itſelf is to be drawn. If theſe difficulties be of importance, they may even be capable of balancing probabilities from

the appearances of minerals; or if, on the other hand, the proof from induction be in any reſpect doubtful or incomplete, ſuch difficulties may be ſufficient to juſtify the rejection of the principle partly aſſumed and partly attempted to be proved. We are not allowed, therefore, to draw the concluſion without regarding theſe difficulties *à priori*, and then ſay they are of no avail, becauſe the proof is already eſtabliſhed.

But, farther, though it were admitted as really proved, that minerals have been formed by fuſion, it is not this ſimple propoſition merely which conſtitutes the Huttonian hypotheſis; it is a detailed ſyſtem, in which a number of ſucceſſive operations are ſuppoſed. If, in the ſeries, one operation can be pointed out, for the uniform production of which proviſion is not made, the ſyſtem is ſubverted, even though other propoſitions which it embraces may be fully proved. From the nature of heat, it can be ſhown that this power cannot be accumulated in the central parts of the globe, to the extent which is neceſſary in the Huttonian doctrine, and that there is no ſource from which it can be regularly ſupplied. Though it were, therefore, proved by indiſputable induction from the qualities of minerals, that they have been formed by fuſion, this would not prove the truth of the Huttonian hypotheſis. The concluſion muſt mere-

ly be admitted, that they have been formed by fire, but not that the fire has been applied in the mode, and under the circumſtances which that hypotheſis points out. Were it even proved, from the poſitions of the ſtrata, that they had been formed and elevated by a ſubterranean heat, this would ſtill not prove the peculiarities of the ſyſtem, that theſe muſt have originated from a former continent; that the materials ariſing from its waſte are ſubjected to a central heat which at all times continues to operate, and that a ſucceſſive elevation of them takes place. In a word, were the igneous origin of foſſils clearly demonſtrated, it would ſerve only to eſtabliſh a Vulcanic theory, but would no more prove the truth of the particular ſyſtem of Hutton, than it would that of Leibnitz or Buffon. It is to no purpoſe, therefore, that we are told by Dr. Hutton, that he has proved that minerals have been formed by fuſion. This, were it granted, does not prove, nor even tend to eſtabliſh, the ſeveral propoſitions which conſtitute his ſyſtem. Some of theſe are, on the contrary, proved to be falſe; and, therefore, were the proof of the firſt propoſition admitted, the refutation of the entire ſyſtem, or the demonſtration of its falſity would ſtill be complete.

Laſtly, when the Huttonian doctrine is tried

by the teſt which Profeſſor Playfair himſelf has properly pointed out, the deciſion muſt be given againſt it. " If," ſays he, " a downright " abſurdity or inconſiſtency with known and " eſtabliſhed facts be involved in any princi- " ple, it ought not to be admitted, however " it may ſeem calculated to explain other ap- " pearances." It is a downright abſurdity to ſuppoſe that caloric could be propagated from the centre of the earth to the bottom of the ſea, ſo as to conſolidate the looſe materials there collected, and ſhould never be able to extend itſelf farther. It is an inconſiſtency with known and eſtabliſhed facts to ſuppoſe that caloric may exiſt in an active ſtate in part of a maſs of matter without being propagated through the whole; and the principle, therefore, grounded on this ſuppoſition, muſt be rejected, however perfect its explanation of phenomena might be. The ſuppoſition itſelf is juſt as abſurd as that which the Profeſſor has ſtated, as ſufficient, if it had been maintained, to overturn the theory, that combuſtion may be carried on where there was no vital air; for we have not more complete evidence eſtabliſhing the truth as to the one of theſe points, than we have as to the other. The fact is as clearly demonſtrated, that caloric diffuſes itſelf over matter till an equilibrium of tempe-

nature is attained, as that the preſence of oxygen is neceſſary for the proceſs of combuſtion; and any principle which contradicts the one muſt be regarded as falſe, as much as that which contradicts the other. We may, therefore, pronounce in the words of Mr. Playfair, "that a "theory founded on ſuch *poſtulata* is worſe "than chimerical."

The extenſive diſcuſſion on this firſt principle of the Huttonian theory, may demand ſome apology. Its importance affords one that is ſufficient. The evidence for a geological theory, from the appearances of minerals, muſt often be imperfect or ambiguous, and it evidently is ſo, ſince different opinions are held on the ſubject. But when the firſt principles of a ſyſtem are proved to involve abſurdities, and contradictions with known facts, the refutation of it is neceſſarily more complete.

The diſcuſſion is of importance likewiſe in the ſubſequent inveſtigation. No hypotheſis was ever broached which did not explain, and explain even in a ſatisfactory manner, ſome of the phenomena it is deſigned to embrace; and this will no doubt be found to hold true of the Huttonian theory. But when the falſity of its principles is thus demonſtrated, theſe explanations will be regarded in their true light—as ſkilful or fortunate adaptations of the hypotheſis to the

phenomena, and not as juſt interpretations of nature.

The laſt general principle of the Huttonian ſyſtem is, that the ſtrata after having been fuſed and conſolidated by ſubterranean heat, had been elevated by the ſame power. To its probability, *à priori*, the ſame objection may be made which has been already urged with reſpect to the fuſion of the ſtrata,—the difficulty or impoſſibility of obtaining and preſerving a degree of heat ſufficient for ſuch a purpoſe. And were this granted, no principle is pointed out in the theory by which the action of this power can be regulated. It is always repreſented as the peculiar excellence of this ſyſtem, that none of its operations are the reſult of accident, but all are adapted to the attainment of a determinate end. Yet in the ſuppoſition of this elevation of the ſtrata by an expanſive power, no cauſe is pointed out for its regular exertion in the mode the theory ſuppoſes, or why it ſhould not be occaſionally the cauſe of havoc and diſorder, as of the renovation of a continent; and why it might not elevate theſe ſtrata before they had been fully prepared. For the production of the effect the Huttonian theory aſcribes to it, it is neceſſary that it ſhould be exerted on the ſtrata, conſolidated and ready for elevation at the bot-

tom of the ſea. Yet there is nothing connected with it, no circumſtance pointed out which ſhould preſerve it within theſe limits, or cauſe it to act there more than upon the elevated land. The principle aſſumed, therefore, is at once gratuitous and improbable. How far it accounts for the appearances which the ſtrata exhibit, is afterwards to be conſidered.

We are now to conſider the Neptunian theory with regard to the probability of its firſt principle,—that the different foſſils have been formed, and the ſtrata arranged by depoſition from water.

The great objection to this principle is, the inſolubility in water of the matter of which theſe ſtrata conſiſt. The ſimple earths which are found in any conſiderable quantity in nature, as lime, argil, magneſia, and ſilex, are very ſparingly ſoluble in it; and the compounds which they form by their union, the different earthy foſſils, are many, or even the greater number of them, apparently inſoluble. How therefore can it be ſuppoſed that water is the agent which has given them fluidity, or that they have been conſolidated from aqueous ſolution? "To affirm "that water was ever capable of diſſolving theſe "ſubſtances, is to aſcribe to it powers which it "confeſſedly has not at preſent; and therefore

" it is to introduce an hypotheſis, not merely " gratuitous, but one which, phyſically ſpeaking, " is abſurd and impoſſible *."

In ſtrict reaſoning, the Neptuniſt may decline anſwering objections of this kind. The only principle which he aſſumes is, that foſſils muſt have been formed by conſolidation from aqueous ſolution, becauſe the appearances they preſent are incompatible with the ſuppoſition of their being formed by fire. If he can eſtabliſh this, his opinion is proved, and his reaſoning may therefore be entirely confined to it. He may reject, as ſubjects beyond the reach of inveſtigation, the inquiries reſpecting the manner in which this aqueous ſolution and the conſolidation from it were effected; and if preſſed with objections from theſe topics, the anſwer is ſufficient, that, in the operations of nature, effects may have ariſen from cauſes, though in the preſent ſtate of our knowledge we may be unable to point out how from ſuch cauſes they could have been produced. The Huttonian geologiſt, by aſpiring to higher ground, occupies a leſs advantageous poſition. He preſents us with a connected ſyſtem, in which not only a particular cauſe is pointed out for the production of certain effects, but in which alſo is detailed how

* Illuſtrations, p. 18.

and where, with what force, and under what modifying circumſtances this cauſe has operated; and conſequently objections to any of theſe parts of his doctrine it is incumbent on him to anſwer. But the Neptuniſt may reſt ſatisfied with the induction from facts, that water has been the chief agent in the formation of minerals, and his concluſion will be juſt, though he may be unable to point out how it has operated.

Let us endeavour to diſcover, however, if the force of this great objection to the Neptunian theory may not be obviated.

It may be remarked, that the reaſoning from which it derives its force is of that faulty kind which a ſound logic proſcribes. We reaſon confidently from what is at preſent to what has been, and ſuppoſe unreaſonably that foſſils muſt have been the ſame at their formation and arrangement as they now are; that they muſt have been in the ſame ſtate of aggregation, muſt have exerted the ſame affinities, and had the ſame relations to each other. Yet nothing is more certain than that in theſe reſpects they muſt have been extremely different, and ſuch differences muſt have produced the moſt important effects.

Thus it is a principle now admitted by every chemiſt, that a ſubſtance in maſs, from its ſtate of aggregation, may be inſoluble in a fluid in

which, when in a ſtate of extreme mechanical diviſion, it may be diſſolved. The examples of this kind in the mineral kingdom are numerous. The corundum ſtone, though conſiſting principally of argillaceous earth, is inſoluble in any acid till its coheſion be overcome. Flint is not acted on by an alkali, unleſs it has previouſly been reduced to a ſtate of mechanical diviſion. The jargon reſiſts every attempt to decompoſe it in the humid way, till its aggregation is overcome by the joint action of potaſh and a ſtrong heat. The native oxyd of tin is from the ſame cauſe inſoluble in any acid. And in general, in mineral analyſis, this inſolubility from aggregation, requires to be overcome by various preliminary operations.

It is apparently from this cauſe that ſeveral of the earths and earthy foſſils have been conſidered as incapable of being diſſolved by water, though ſoluble in that fluid. This is particularly exemplified in ſilex, the earth which has been deemed the leaſt ſoluble of any, and of courſe with reſpect to which the Neptuniſt finds the greateſt difficulty. When triturated with water, no ſenſible portion of it is diſſolved; yet there are a variety of facts which prove it to be ſoluble. If, for example, the ſilex be combined by fuſion with an alkali, and if this compound be afterwards decompoſed by an acid, the ſili-

ceous earth thrown down is in a ſtate of extreme mechanical diviſion, into which it cannot be brought by any other means; and in this ſtate it is ſoluble in water; ſo that if a large quantity be employed to dilute it, the earth is not precipitated. Siliceous ſtalactites, though rare, are ſometimes however found, as is admitted by Dr. Hutton himſelf; and in nature, ſilex has actually been found diſſolved in many mineral waters. It is ſufficient to give as an example of this the water of the Geyſer fountain in Iceland. A hundred cubic inches of this water were found by Klaproth to hold diſſolved not leſs than nine grains of ſilex, and by Dr. Black 10.8 grains. The latter illuſtrious chemiſt ſuppoſed that the ſolubility of the earth might be promoted by a portion of ſoda likewiſe preſent in the water. But Klaproth has juſtly obſerved, that the quantity of this is comparatively ſo ſmall, (only three grains according to his analyſis, and 1.5 according to Dr. Black's, in 100 cubic inches of the water) that it is altogether inadequate to the production of ſuch an effect; and that, beſides this, the ſoda is neutraliſed by carbonic acid, while it is only the pure alkali that promotes the ſolubility of this earth. Silex has alſo been found diſſolved in water in which no alkali was preſent *. No

* Kirwan's Geological Eſſays, p. 117.

doubt therefore can remain of the juſtneſs of Klaproth's opinion, that this earth exiſts in the water of this fountain, from its natural ſolubility in that fluid, aſſiſted probably by the high temperature, ſince we find it depoſited at the ſides of the fountain; a depoſition which muſt be owing to the temperature of the ſpring being reduced on expoſure to the air.

It is proved, therefore, that ſiliceous earth is ſoluble in water, and that its apparent inſolubility is owing merely to its ſtate of aggregation. It follows, that the ſame cauſe muſt be aſſigned for the inſolubility of other foſſils; and, of courſe, that, though now inſoluble from their ſtate of aggregation, they may ſtill have had an aqueous origin. To thoſe with whom an example is more convincing than reaſoning, this will be rendered evident by a ſimple fact. The ſiliceous depoſition at the Geyſer fountain, is compoſed, according to Klaproth, of 98 of ſilex, 1.5 of argil, and 0.5 of oxyd of iron; it is frequently as hard as agate, and is inſoluble in water, as much as any earthy foſſil. Yet it owes its formation to aqueous ſolution; a fact ſufficient to convince us of the error of the concluſion, that foſſils cannot be formed by water, becauſe, after their conſolidation, we find them not ſoluble in that fluid. It is, indeed, an actual demonſtration, that ſuch a concluſion is falſe.

Farther, the ſolvent power of water, with reſpect to any ſubſtance, is invariably promoted by heat. If we conceive, that, at the commencement of the formation of our ſtrata, this fluid held in ſolution a great quantity of ſaline, earthy, and metallic matters, it is certain that it would be capable of ſuſtaining a much higher temperature than pure water would; and this high temperature, it is indubitable, would augment its ſolvent power. It is nearly demonſtrated, that, by this agent, the power of combination, of which ſolution is merely a particular caſe, may be increaſed to any requiſite extent. The reſearches of Berthollet on chemical affinity, and a number of facts recently eſtabliſhed, have rendered it extremely probable, that there are no two bodies in nature which have not attractions to each other; that water, for example, has not only an attraction to many ſubſtances, but to all; and that, in particular caſes, exiſting attractions are prevented from being efficacious, only by the aggregation in the bodies concerned being ſuperior to the chemical attraction exerted between them, or to other circumſtances preventing their union. The former power, aggregation, is uniformly diminiſhed by heat; and it follows nearly as a corollary, that, by its action applied in a ſufficient degree, any two bodies may be made to

combine, or any fluid may be made the ſolvent of any ſolid. Experiment likewiſe eſtabliſhes this concluſion; for, when water is made to ſuſtain a high heat, it becomes the ſolvent of ſubſtances, of which, at a low temperature, it appears to be incapable of diſſolving even the moſt minute portion.

Now, it is no improbable ſuppoſition, that, at the period when the materials of the ſurface of our globe were in ſolution, the temperature may have been much ſuperior to that which is at preſent neceſſary for the operations of nature. There muſt, neceſſarily, have been aſſigned to this planet, a certain quantity of caloric, and this, before that order was eſtabliſhed which now prevails, may have been locally accumulated, and may thus have been capable of producing, at the ſurface, the greateſt effects. It has alſo been the opinion of ſeveral geologiſts, (and, as an hypotheſis, there is nothing to prevent it from being aſſumed) that, at this period, the atmoſphere was not formed; of courſe, the immenſe quantity of latent heat which it now contains, would be ſenſible and active in the fluid maſs; and, from this cauſe alone, its temperature muſt have been high. It is needleſs to repeat, that, from ſuch a cauſe, the extent of which it is not eaſy to aſſign, it is impoſſible to calculate the effects; and he who would limit

them, who would ſay that ſuch a power could not be preſent, or that it was incapable of the operations ſuppoſed, would reaſon in oppoſition to principles eſtabliſhed by indubitable evidence. This aſſertion would even ſtand contradicted by facts; for, the ſolution of ſilex in the Bath waters, Carlſbad waters, thoſe of the Geyſer fountain, and other hot ſprings, prove the reality of ſuch a power in heat, and that it is capable of producing ſuch effects.

It is now likewiſe admitted, that it is impoſſible to determine the force of a chemical attraction between any two bodies, or the effect which would ariſe from its exertion, otherwiſe than by obſervation or experiment. And the propoſition, independent of all theory, would at preſent receive the aſſent of every chemiſt, that, if a number of ſubſtances were brought into contact, in a ſtate of extreme diviſion, by the medium of a fluid, which itſelf exerted attractions to many of them, it would be impoſſible to eſtimate what attractions would be moſt efficacious, what would be the reſult of their concurrent exertion, or what might be ſuppoſed to be the effect ariſing from the preſence of any of them.

To ſpeak more preciſely with regard to the preſent queſtion, it is ſuppoſed in the Neptunian theory, that at the period of the formation of our

ſtrata, there were diſſolved in the chaotic fluid, the different ſimple earths; the ſimple inflammables, the metals; with various ſaline matters not decompoſed, or if theſe are compounds, the elements of which they conſiſt. To theſe, moſt probably, are to be added the elaſtic fluids, which at preſent form our atmoſphere, and all thoſe principles which are now accumulated in the animal and vegetable ſyſtems. By what power were theſe ſubſtances held in ſolution by the water? The Huttonian, in his argument againſt the Neptunian hypotheſis, replies, that it muſt be ſuppoſed to be by the attraction of each of them ſeparately to that fluid. But the reply is abſurd. Each ſubſtance would exert an attraction more or leſs efficacious to every other preſent, or at leaſt, according to the old chemical notion, it would exert attractions to many of them; and the effect which would reſult from theſe complicated attractions, it is impoſſible to eſtimate. The number of ſimple ſubſtances found in nature, and which muſt of courſe have been all preſent in this fluid, exceeds forty. Suppoſe the half of theſe to be in ſuch ſmall quantities as to be imperceptible in their action, ſtill, from the numerous attractions of the others, any imaginable effect might ariſe.

To illuſtrate this poſition, if we take one of

theſe ſubſtances, ſuppoſe the oxygen, which now conſtitutes one fourth of our atmoſphere, and which enters into the compoſition of all the vegetable and animal ſubſtances at preſent exiſting in ſuch abundance, how are we to determine what may have been the effects reſulting from the exertion of its attractions, when deprived of its elaſticity? To ſpecify one of them, it is capable of combining with all the metals; and it not only promotes their ſolubility in water, but increaſes the force of their attractions to the earths, or the facility of their combination with them. How, therefore, are we to follow it through theſe numerous combinations, ſo as to determine the ultimate general effect? Or take hydrogen as an example: it is capable of combining with ſulphur, and with carbon; and theſe compounds again become ſolvents of other matters. Hydrogen is even capable, in its naſcent ſtate, of diſſolving ſome of the metals, and when deprived of its elaſticity, or preſented in a ſtate of condenſation, it is poſſible it might be capable of forming combinations with the whole of them. Or, laſtly, let us take as our illuſtration the muriat of ſoda, which exiſts in ſuch immenſe quantity in nature: It is evident, that if we ſuppoſe its principles, the acid and the alkali compounds, as their ſimple elements are unknown to us, we introduce the operations of ſubſtances, which, ſo far as we know, may

be fully adequate of themſelves to effect the ſolution of every kind of matter in the chaotic fluid: Or if, as they have not been decompoſed, we ſuppoſe them ſimple, we are not to imagine that they would exiſt in a mere binary combination, or as muriat of ſoda; their attractions would be divided and modified by the other ſubſtances preſent, in ſuch a manner, that he would expoſe himſelf to the charge of ignorance of chemiſtry, who would venture to foretel the reſult. When to theſe three natural ſubſtances which we have taken as examples, the affinities of ſo many others are added, it is obvious, that, from ſuch complicated attractions, any effect which implies not a phyſical impoſſibility might be produced.

Another clear illuſtration of the ſame truth is afforded by a ſingular fact which has been ſtated, and which on experiment will be found to be juſt. When the alkaline ſolutions of ſilex and argil are mixed together in equal proportions, "a firm, gelatinous, opaleſcent maſs, re-
"ſults in a very few minutes. This is perfect-
"ly inſoluble in water, yet ſoluble in acids,
"whether concentrated or diluted, nay even in
"diſtilled vinegar, and yet conſiſts of both ſi-
"lex and argil: Here, therefore, the properties
"of the ſilex muſt be conſiderably altered *."

* Nicolſon's Journal, vol. iv. p. 543.

No one could have imagined, *à priori*, that argil could render ſilex ſoluble in acids, in which by itſelf it is perfectly inſoluble; and from this ſingle fact it is obvious, that the concluſion is equally probable, that another ſubſtance, by the attraction it exerts to ſilex, might render it ſoluble in water. It places, indeed, in a clear light, the influence ariſing from attractions, which we ſhould not have ſuppoſed would be important; and fully proves, that the effects which may reſult from the mutual chemical actions of a number of ſubſtances, cannot poſſibly be eſtimated. It is a fact of the very firſt importance, and which cannot be too highly prized by the Neptunian geologiſt.

Reaſoning of this kind, though in itſelf ſufficiently concluſive, is always rendered more forcible when it can be ſhown, that effects, analogous to what are ſuppoſed, actually exiſt in nature, and ariſe from cauſes of a ſimilar kind. In the preſent caſe we can obtain the advantage of ſuch an illuſtration. In the formation of the animal and vegetable ſubſtances, affinities are exerted of which we have no knowledge, but from the products that are formed, and combinations take place which we could not have imagined, and which we cannot imitate. A few ſimple ſubſtances, carbon, hydrogen, oxygen, azote, and phoſphorus, only are

concerned; but ſo far from forming the few binary, or ternary compounds which art can produce by combining them, they form an innumerable variety of compounds, diſtinguiſhed by the poſſeſſion of the moſt oppoſite properties. To what cauſe is this to be referred? The phyſiologiſt may content himſelf with aſcribing it to the ſuperintending influence of an unknown vital power. But if we are not to ſatisfy ourſelves with a term deſtitute of meaning, and which, ſo far from ſolving the problem, conveys no preciſe idea, we will ſearch for ſome more intelligible theory. We will find it in the circumſtances in which theſe ſubſtances are placed. We now know that chemical affinity is not to be regarded as an abſolute power, which, in all caſes where it can operate, will do ſo with an equal force. It is moſt materially influenced in its action by a variety of circumſtances; by force of aggregation, diſtances of the particles, temperature, elaſticity or condenſation of the agents concerned, quantity of the maſs, and others perhaps not aſcertained. Wherever theſe are varied, eſpecially where ſeveral ſubſtances, having mutual attractions, are preſent, a difference will take place in the affinities exerted, and thus, from a very ſlight variation of circumſtances, the moſt varied combinations will reſult. Art cannot eaſily pro-

cure or regulate ſuch variations, and ſhe therefore cannot imitate the operations of animated nature, in which, by the moſt complicated arrangement, the moſt ample proviſion is made for the attainment of theſe modifying powers. Hence, as has been ſaid, effects reſult on which we could not, *à priori*, calculate; and the inference from this, to theſe operations by which the materials of our ſtrata were diſſolved and conſolidated, is direct and incontrovertible. Attractions muſt have been exerted, of which we know neither the number nor abſolute force; and theſe muſt have been indefinitely modified, by the circumſtances of temperature, quantity, condenſation, and others which we cannot determine, and which, in the inſtance of the organic products now pointed out, produce ſuch important effects. He, therefore, would reaſon ſtrangely, who would abſtract entirely theſe circumſtances, and tell us, that becauſe when flint is triturated in a mortar with water it is not diſſolved, it is a phyſical impoſſibility that ſiliceous earth ſhould have been in ſolution in the chaotic fluid. His reaſoning would be nearly the ſame as that by which it ſhould be affirmed, that ſugar cannot be a compound of carbon, hydrogen, and oxygen, becauſe we cannot form it by combining theſe principles, or

are unable, even by any means, to bring them into a ternary combination.

Laſtly, we do not know what really are the ſimple principles of the ſubſtances exiſting in the mineral kingdom; and this of itſelf is ſufficient to ſolve the whole difficulty. If we analyſe an earthy foſſil, we find our analyſis cannot be carried beyond certain ſubſtances, which we term ſimple earths: or if we examine the compoſition of an ore, we reſolve it into a metal combined with ſome other body. But can we affirm that theſe earths and metals are certainly ſimple bodies, or that they may not be compounds? He who knows any thing of the principles of chemiſtry, or even of its hiſtory, would never conſider the former concluſion as eſtabliſhed, but would rather incline to the latter, ſince our analyſis can never prove a body to be abſolutely ſimple; and the progreſs of diſcovery has invariably been, to prove ſubſtances, apparently ſo, to be compounds, and compounds too of a nature very different from what the ſtate of knowledge prior to the diſcovery would have ſuggeſted. If ſome years back an opinion on any ſubject had been maintained which required the ſuppoſition that water and air were compounds, it would probably, from the prevalent notion of theſe bodies being elements, have been rejected, yet the event has ſhown

that it would have been juſt. And on the ſame grounds, it is equally allowable at preſent to ſuppoſe, if it be neceſſary to do ſo to ſolve a difficulty attending a principle eſtabliſhed on evidence, that the earths and metals may be compounds; and indeed in itſelf ſuch a ſuppoſition is entitled to equal regard with that which conſiders them as ſimple, and is perhaps more conformable to truth. As a general rule to guard againſt extravagant ſpeculation, or as a principle which regulates our arrangements, it may not be improper to rank every ſubſtance as ſimple which is not decompoſed; but it ſhould not be forgotten, that this is a mere ſuppoſition, admitted for theſe reaſons, that it is only a principle of convenience, but that in the abſtract it is not more certain than the oppoſite concluſion.

We ſhould not indeed err much, perhaps, if we conſidered the greater number of bodies which are at preſent the ſubjects of our knowledge as compounds. Chemiſtry is but in its infancy; within a few years only has it diſcovered the compoſition of a number of ſubſtances; and ſhall we believe that it has already attained the end of its reſearches, and that the varieties of matter which analyſis has diſcovered are truly elementary? Bodies, as they approach to ſimplicity, apparently become more ſubtle,

as is evident from contrafting the ſimple gaſes with the compounds they form. When we compare the amazing ſubtility and tenuity of light, and thoſe kinds of matter which give riſe to the phenomena of magnetiſm, electricity, and galvaniſm, with the groſſneſs and fluggiſhneſs of the metals and earths, or even the ſimple gaſes, we will perceive that here the uſual gradations of nature are not obſerved; that the chain which ſhould connect material bodies is as it were broken between theſe different claſſes; and that the ſuppoſition is not improbable that theſe groſſer bodies are compounds of others more ſubtle, which may approach, or graduate into thoſe kinds of matter that have the moſt undoubted claim to the character of ſimplicity. There are likewiſe a variety of facts both in chemiſtry and mineralogy, ſuch as the apparent tranſmutation of flint into calcareous earth, and the production of the earths in the vegetable ſyſtem, which appear to indicate that theſe are compounds.

To ſuch ſuppoſitions, however, though in themſelves ſufficiently probable, and not unſupported, we need not have recourſe. It is ſufficient that the abſolute ſimplicity of theſe bodies,—the earths and metals, is not proved. If the poſſibility of their being compounds be admitted, (and its impoſſibility the Huttonian

will never be able to demonſtrate) the objection to the Neptunian theory of their formation, that they are nearly inſoluble in water, is of no force. It is obvious, that if compounds, their principles might exiſt in the fluid or even aerial form, that they might be ſoluble in water, or by their mutual attraction might contribute reciprocally to their ſolution, and that when combined in other modes they might form thoſe leſs ſoluble compounds which now exiſt.

When we conſider the circumſtances now enumerated, the influence of aggregation in preventing ſolution, the power of temperature in promoting it, the incalculable effects reſulting from the exertion of complicated affinities, and the poſſibility of ſubſtances being compounds, which our imperfect knowledge ranks as ſimple, we can have no heſitation in admitting the concluſion which each ſeparately eſtabliſhes, that foſſils may have been formed by water, though apparently inſoluble in that fluid. And if an induction from facts ſhall render probable their aqueous origin, their preſent inſolubility will form no objection of real force.

Dr. Hutton has likewiſe ſuppoſed, that, granting the ſolubility in water of the matter of which our ſtrata conſiſt, their ſubſequent conſolidation cannot be accounted for on the Neptunian theory. The poroſity of the maſs could never be

entirely baniſhed; and had minerals been conſolidated in this way, the ſolvent ought either to remain in them in a liquid ſtate, or if ſeparated by evaporation or percolation, it muſt have left the pores empty, and the body pervious to water.

" Water," ſays Dr. Hutton, " being the general medium in which bodies collected at the " bottom of the ſea are always contained, if " thoſe maſſes of collected matter are to be conſolidated by ſolution, it muſt be by the diſſolution of thoſe bodies in that water as a " menſtruum, and by the concretion or cryſtallization of this diſſolved matter, that the ſpaces, " firſt occupied by water in thoſe maſſes, are afterwards to be filled with a hard and ſolid ſubſtance; but without ſome other power, by " which the water contained in thoſe cavities " and endleſs labyrinths of the ſtrata, ſhould be " ſeparated in proportion as it had performed its " taſk, it is inconceivable how thoſe maſſes, " however changed from the ſtate of their firſt " ſubſidence, ſhould be abſolutely conſolidated, " without any viſible or fluid water in their " compoſition.

" Beſides this difficulty of having the water " ſeparated from the porous maſſes which are to " be conſolidated, there is another with which, " upon this ſuppoſition, we have to ſtruggle.

" This is, From whence ſhould come the mat-
" ter with which the numberleſs cavities in
" thoſe maſſes are to be filled?

" The water in the cavities and interſtices of
" thoſe bodies compoſing ſtrata, muſt be in a
" ſtagnating ſtate; conſequently, it can only
" act upon the ſurfaces of thoſe cavities which
" are to be filled up. But with what are they
" to be filled? Not with water; they are full
" of that already: Not with the ſubſtance of
" the bodies which contain that water; this
" would be only to make one cavity in order
" to fill up another. If, therefore, the cavities
" of the ſtrata are to be filled with ſolid matter,
" by means of water, there muſt be made to
" paſs through thoſe porous maſſes, water im-
" pregnated with ſome other ſubſtances in a diſ-
" ſolved ſtate; and the aqueous menſtruum muſt
" be made to ſeparate from the diſſolved ſub-
" ſtance, and to depoſit the ſame in thoſe cavi-
" ties through which the ſolution moves*."

Profeſſor Playfair adds to theſe obſervations,
" It is evident, that the conſolidation pro-
" duced by the action of water, or of any other
" fluid menſtruum, in the manner juſt referred
" to, muſt neceſſarily be imperfect, and can ne-
" ver entirely baniſh the poroſity of the maſs.

* Theory of the Earth, vol. I. p. 44, 45.

" For the bulk of the ſolvent, and of the mat-
" ter it contained in ſolution, being greater
" than the bulk of either taken ſingly, when
" the latter was depoſited, the former would
" have ſufficient room left, and would continue
" to occupy a certain ſpace in the interior of
" the ſtrata. A liquid ſolvent, therefore, could
" never ſhut up the pores of a body, to the en-
" tire excluſion of itſelf; and, had mineral ſub-
" ſtances been conſolidated, as here ſuppoſed,
" the ſolvent ought either to remain within
" them in a liquid ſtate, or, if evaporated, ſhould
" have left the pores empty, and the body per-
" vious to water. Neither of theſe, however,
" is the fact; many ſtratified bodies are per-
" fectly impervious to water, and few mineral
" ſubſtances contain water in a liquid ſtate.
" That they ſometimes contain it, chemically
" united to them, is no proof of their ſolidity
" having been brought about by that fluid;
" for ſuch chemical union is as conſiſtent with
" the ſuppoſition of igneous as of aqueous con-
" ſolidation, ſince the region in which the fire
" was applied, on every hypotheſis, muſt have
" abounded with humidity *."

Theſe objections, it may be affirmed, ariſe from aſcribing an hypotheſis to the Neptunian

* Illuſtrations of the Huttonian Theory, p. 16, 17.

theory which does not belong to it, and that, with reſpect to this point, it labours under no real difficulty. This will be evident, from ſtating briefly the modes in which it ſuppoſes conſolidation to take place from fluidity by water.

If the ſolid ſubſtance be completely diſſolved in the water, it may ſeparate from it by cryſtallization, this cryſtallization being determined by a change of circumſtances in the ſolution. If the ſolubility of the ſolid had been augmented by the heat of the ſolvent, or by the preſence in it of certain principles, ſuppoſe of an aerial kind, it is evident, that on a reduction of temperature, or on the eſcape of theſe principles, or even on new combinations taking place, cryſtallization would commence: it would proceed more or leſs rapidly according to the extent in the change of circumſtances bringing it about; and in this manner might be formed the different ſtrata which exhibit marks of cryſtallization in their ſtructure. No one can doubt that in this way maſſes of the greateſt hardneſs and denſity might be formed, and the chemiſt indeed has daily opportunities of obſerving the hardneſs which a cryſtalline maſs acquires, when it ſtands for ſome time with the liquor over it from which it had been depoſited, every vacuity which may have been left in it at its firſt cryſtallization being filled up by ſubſe-

quent cryſtalline depoſition, and the fluid filling theſe cavities, being extruded.

Other ſtrata may have been formed merely by depoſition of ſolid matter, not diſſolved, but ſuſpended in a fluid. In this manner have apparently originated a conſiderable number of the ſecondary ſtrata. The particles, depoſited in large beds, would at firſt have little coheſion; but there is no improbability in the ſuppoſition, that in this ſoft ſtate the attraction of aggregation would be exerted between them, ſince we know that neither this ſpecies of attraction, nor chemical affinity, is entirely confined in its action to the ultimate particles of matter, but can be exerted between minute maſſes. The particles would thus approximate, the interpoſed fluid would be forced out, and a maſs more or leſs compact would be formed. This, on remaining a long time at reſt, might attain a great degree of hardneſs; and it may be obſerved, that in nature no foſſil, at leaſt none uncryſtallized, is ſo perfectly denſe as to be altogether impervious to water. This ſpecies of conſolidation would be ſtill more promoted if a chemical were mixed with the mechanical depoſite, or matter was ſeparated that had been diſſolved, at the ſame time with the ſubſidence of the particles ſuſpended, a circumſtance which appears to have often happened.

The futility of Dr. Hutton's objections to this account of conſolidation from aqueous ſolution will now be apparent. They do not indeed apply to it, but to an hypotheſis which no Neptuniſt maintains. In accounting for the conſolidation of the ſtrata by water, it is not conceived that the looſe materials are firſt depoſited at the bottom of the ſea, and that then ſome matter is introduced into their pores by which they are rendered hard. They have been formed either by cryſtallization or precipitation. In the former mode it is obvious that no operation of this kind can be ſuppoſed requiſite to conſolidate them; in the latter caſe it is in general equally unneceſſary, becauſe when the particles in a ſtate of minute diviſion were depoſited, they might cohere, and would not, as Dr. Hutton conceives, require the introduction of any other matter to complete their conſolidation. But were even ſuch a ſuppoſition made, the objection would be groundleſs. If the water ſuppoſed to be introduced into the precipitated maſs had particles of ſolid matter merely *ſuſpended* in it, theſe would be diſpoſed to ſubſide, and cohere more or leſs firmly with the ſolid matter into which the water had been inſinuated, and the water itſelf would at the ſame time be extruded. Or if the ſolid matter were *diſſolved*, it would be diſpoſed to cryſtallize, by obtaining a nucleus

in the ſtrata into which the water containing it had inſinuated. There are even facts which fully prove that ſtony particles may be introduced by infiltration into ſtrata, and may give them additional hardneſs *, a cauſe which, it is not improbable, has operated to a conſiderable extent in mineral conſolidation.

Still Dr. Hutton repeats his objections to the Neptunian explanation of conſolidation. In his reply to Mr. Kirwan, who had ſtated this explanation, he has the following ſingular obſervations: "If I underſtand our author's (Mr. "Kirwan's) argument, the particles of ſtone "are, by their mutual attractions, to leave thoſe "hard and ſolid bodies which compoſe the "ſtrata, that is to ſay, thoſe hard bodies are to "diſſolve themſelves; but, To what purpoſe? "This muſt be to fill up the interſtices, which "we muſt ſuppoſe occupied by the water. In "that caſe, we ſhould find the original inter-"ſtices filled with the ſubſtances which had "compoſed the ſtrata, and we ſhould find the "water tranſlated into the places of thoſe "bodies; here would be properly a tranſmuta-"tion, but no conſolidation of the ſtrata, ſuch "as we are to look for, and ſuch as we actually "find among thoſe ſtrata. It may be very

* Kirwan's Geological Eſſays, p. 132.

" eaſy for our author to form thoſe explanations " of natural phenomena; it coſts no tedious " obſervation of facts, which are to be gathered " with labour, patience, and attention; he has " but to look into his own fancy, as philoſo- " phers did in former times, when they ſaw " the abhorrence of a vacuum and explained " the pump. It is thus that we are here told " the conſolidation of ſtrata *ariſes from the mu-* " *tual attraction of the component particles of* " *ſtones to each other*. The power, by which the " particles of ſolid ſtony bodies retain their " places in relation to each other, and reſiſt ſe- " paration from the maſs, may, no doubt, be " properly enough termed their mutual attrac- " tions; but we are not here inquiring after " that power; we are to inveſtigate the power " by which the particles of hard and ſtony " bodies had been ſeparated, contrary to their " mutual attractions, in order to form new con- " cretions, by being again brought within the " ſpheres of action in which their mutual at- " tractions might take place, and make them " one ſolid body. Now, to ſay that this is by " their mutual attraction, is either to miſunder- " ſtand the proper queſtion, or to give a moſt " prepoſterous anſwer *."

* Theory of the Earth, vol. i. p. 228, 229.

The cauſe of this ſingularly confuſed and miſtaken ſtatement, appears to be, Dr. Hutton having always before him a limited view of his ſubject. When he aſks, by what cauſe are the ſtrata conſolidated? he appears to mean, not as his expreſſion would imply, the ſtrata in general, but thoſe particular ſtrata which conſiſt of mechanical fragments cemented together. He conſiders theſe ſtrata as compoſed of particular collections of ſubſtances, limeſtone, for example, of remains of marine animals, and pudding-ſtone, of pebbles or gravel, which are conſolidated by being fixed in a common cement. When, therefore, his queſtion is anſwered, by ſaying, that conſolidation is owing to the mutual attraction of the particles, the anſwer appears to him prepoſterous. But this is owing to the prepoſterous manner in which he has conceived the ſubject. It is a proper anſwer with reſpect to the conſolidation of homogenous ſtrata, which amount at leaſt to nine in ten of the whole. And, if the queſtion were farther put, with reſpect to theſe particular ſtrata, which are compoſed of mechanical fragments cemented together, the anſwer of the Neptunian would then be, that the cementing matter of theſe ſtrata had been depoſited around theſe fragments from ſolution or ſuſpenſion in water, and that the attraction of aggregation being exerted

between its particles, it had conſolidated, and, of courſe, had conſolidated the whole maſs.

Or rather the limited view which Dr. Hutton takes of this ſubject, ariſes from his hypotheſis, that all the ſtrata have ariſen from the decay of former ones; and that, therefore, their materials have once conſiſted of mechanical fragments looſely depoſited. He ſhould have recollected, however, that this ſuppoſition is not admitted by his antagoniſt, and that he therefore was not at liberty to attribute it to him, or to reaſon from it as if it were received. The opinion of the Neptuniſt as to the original condition of the materials of the ſtrata is altogether different, and it is not to this opinion, but to an hypotheſis, the creature of his own imagination, and of his limited views, that the objections of Dr. Hutton on conſolidation apply.

Examples of both the kinds of conſolidation pointed out, are abundant in nature. Calcareous cryſtals and ſtalactites, and even ſiliceous ſtalactites, are frequently formed by water at the ſurface, and ſhow, at leaſt, that from cryſtallization, perfect conſolidation may take place; a concluſion, indeed, which no one can doubt. Of depoſition, too haſty to admit of cryſtallization, the ſiliceous incruſtation at the Geyſer fountain is a ſufficient example. Its hardneſs is frequently ſuch as to be equal to that of the

more hard ſiliceous foſſils, agate, or chalcedony. Many other examples of conſolidation from theſe cauſes, as well as from mechanical depoſition, are ſtated by Mr. Kirwan *. One of theſe may be ſelected,—that of the regeneration of granite, or the formation of a hard ſtone from the materials of that foſſil. It occurred in a mole conſtructed in the Oder, the ſides of which were of granite, and the middle ſpace was filled with granitic ſand. This concreted into a ſubſtance ſo hard as to be impenetrable by water. The mode in which Dr. Hutton attempts to obviate this fact could ſcarcely be explained but in his own words: "Here is an example, according to our author, of granite formed in the moiſt way. But now I muſt aſk to ſee the evidence of that fact; for, from what our author has told us, I do not even ſee reaſon to conclude, that there was the leaſt concretion, or any ſtone formed at all. A body of ſand will be *ſo compacted as to be impenetrable by water*, with the introduction of a very little mud, and without any degree of concretion. Muddy water indeed cannot be made to paſs through ſuch a body without compacting it ſo; and this every body finds to their coſt, who have attempted to make a filter of that kind †."

* Geological Eſſays, Eſſay iv.

† Theory of the Earth, vol. i. p. 267.

This requires no anſwer. It is admitted, that the ſubſtance thus formed, was ſo hard as to be impenetrable by water. (Mr. Kirwan has ſince added, that it was even ſo compact that it could ſcarce be ſeparated from the real granite to which it was contiguous, by a blow). Yet Dr. Hutton ventures to tell us, that this hardneſs is not from the particles having concreted, but from their having been compacted by mud. In another part of his work, he gives diſtinctions, which, he informs his readers, are not for the vulgar to underſtand. This diſtinction between *compacting* and *concreting*, is perhaps one of this kind, and he who underſtands it ſo as to apply it to the ſolution of the preſent difficulty, may congratulate himſelf on being none of the vulgar, but probably a profound logician of Dr. Hutton's ſchool.

If foſſils have been conſolidated from water, it might be ſuppoſed that they ſhould contain a portion of that fluid. The anſwer is, that they actually do ſo. No aggregate perhaps exiſts in nature, which does not loſe weight on expoſure to heat, from the eſcape of water; ſome of the moſt ſolid rocks loſe conſiderably, and ſeveral of the hardeſt minerals contain 5 or 10 parts in the 100. The compoſition of foſſils, therefore, ſufficiently accords with the ſuppoſition of their

being conſolidated by concretion from water; nor has it been ſhown that they ought to contain more water than they actually do. In all cryſtallized bodies, the quantity retained is different, and is dependent partly on their attraction to the fluid, and partly on circumſtances attending their cryſtallization. And there are ſubſtances actually depoſited from water which do not contain any ſenſible portion of it: Such, according to the analyſis of Klaproth, is the ſiliceous incruſtation of the Geyſer fountain.

We may now contraſt theſe two ſyſtems with reſpect to the probability of their firſt principles, and compare the explanation which either affords, with that which the nature of the ſubject admits. We have ſeen, not only that the Huttonian ſyſtem labours under inſurmountable difficulties, in accounting for the production of that heat by which it ſuppoſes the ſtrata to be fuſed and conſolidated, but it has been proved, that its principles ſtand in direct oppoſition to eſtabliſhed facts, and are therefore falſe. The Neptunian ſyſtem, again, in accounting for the original fluidity of the ſurface of the globe from the operation of water, may appear to be liable to ſome difficulties; but it is fertile in reſources, ſince cauſes can be pointed out, by which the

action of that fluid might be modified, ſo as to produce the effects aſcribed to its operation. It may be impoſſible to determine which of the cauſes pointed out did actually operate, or to which the greateſt ſhare in the ultimate effect is to be attributed. But what is to be expected from the geologiſt in ſuch reſearches? not ſurely that he is to point out the preciſe manner in which theſe immenſe operations have been conducted, or, as if he had been a ſpectator of them, deſcribe them in detail. The theoriſt may make pretenſions of this kind, but, under the name of a ſyſtem, he will give us merely a dream. It is ſufficient if, from induction, a particular cauſe be eſtabliſhed, and if this cauſe be rendered probable, or objections to it obviated by reaſoning which involves no improbable ſuppoſitions. This is attained in the Neptunian theory. From the appearances of minerals, we ſhall find ſufficient reaſon to conclude, that they have been formed by water; and the objections which might, *a priori*, be made to this concluſion, from the powers of water being inadequate to ſuch effects, we have ſeen are ſufficiently obviated, by the admiſſion of certain modifying circumſtances, which not only might have operated, but which muſt have done ſo to a certain extent. There is, therefore, nothing to

prevent the admiſſion of the principle which induction eſtabliſhes.

On this ſubject too, were it neceſſary, the principle might be inſiſted on, that, if a certain cauſe be ſuggeſted by the phenomena, it ought to be admitted, though there are difficulties in explaining its preciſe mode of operation. Dr. Hutton and Profeſſor Playfair apply this argument to the ſupport of their doctrine, without perceiving or acknowledging that it can be brought with equal force to eſtabliſh the Neptunian theory: "I have proved," ſays Dr. Hutton, "that thoſe ſtony ſubſtances have been "in the fluid ſtate of fuſion; and from this I "have inferred the former exiſtence of an in- "ternal heat, a ſubterraneous fire, or a certain "cauſe of fuſion, by whatever name it ſhall be "called, and by whatever means it ſhall have "been procured. Though I ſhould confeſs my "ignorance with regard to the means of pro- "curing fire, the evidence of the melting ope- "ration, or former fluidity of theſe mineral "bodies, would not be thereby in the leaſt di- "miniſhed *." It is obvious, that the whole of this argument may be adapted with equal juſtice to the Neptunian theory. I have proved, (a defender of that ſyſtem might ſay) that theſe

* Theory of the Earth, vol. i. p. 239.

ſtony ſubſtances have been in the fluid ſtate of ſolution; and from this I have inferred the exiſtence of a ſolvent, or a certain cauſe of ſolution, by whatever name it ſhall be called, and by whatever means it ſhall have been procured; and though I ſhould confeſs my ignorance with regard to the means of procuring this ſolvent, the evidence of the diſſolving operation, or former fluidity of theſe mineral bodies, would not be thereby in the leaſt diminiſhed.

This mode of reaſoning can indeed be employed with much more force and juſtice by the Neptuniſt than by the Huttonian; for, as has already been ſhown, though it were demonſtrated that minerals had been formed by fuſion, this would not eſtabliſh the truth of the Huttonian ſyſtem, becauſe that ſyſtem involves principles phyſically impoſſible, and which therefore no induction can eſtabliſh; while on the contrary, the Neptunian is at moſt liable to ſome difficulties which are eaſily removed, and which, though they were not, would not be ſufficient to invalidate the evidence which the appearances of minerals afford. Little doubt, therefore, it is preſumed, can remain of the decided ſuperiority of the firſt principles of the one to thoſe of the other. We have next to examine what ſupport theſe theories derive from the phenomena of geology. The arguments of this kind which

have been advanced may be conſidered under two claſſes; thoſe drawn from the poſitions and connections of the ſtrata of the earth, and theſe from the properties and appearances of individual foſſils.

PART III.

Of the Arguments in ſupport of the Huttonian and Neptunian Theories, from the Poſitions *of the Strata of the Globe.*

THE immenſe maſſes which compoſe the ſurface of the earth are extremely various in their poſitions. Some are altogether irregular, others are diſpoſed in beds or ſtrata which are vertical, horizontal, or inclined at different angles to the horizon, varying in their extent and thickneſs, alternating often with each other, and frequently intimately connected at their junctions. Theſe are facts of the firſt importance in geology, and which require to be explained in any theory of the earth.

The Huttonian explanations of the formation of the ſtratified rocks, and of that of the unſtratified, are very different from each other, and require ſeparate conſideration.

Of the STRATIFIED ROCKS it has juſt been obſerved, that ſome are vertical in their poſition, ſome horizontal, or nearly ſo, and others inclined at different angles to the horizon. Dr. Hutton ſuppoſes that all theſe have originally been horizontal, that the matter of which they conſiſt has been ſpread out on the bottom of the ocean, conſolidated by fuſion by ſubterraneous heat, and afterwards elevated by the expanſive power of that heat, ſo as to aſſume the poſitions they are found to have.

In the Neptunian ſyſtem it is ſuppoſed that the poſitions of the ſtrata have been determined, partly by the figure of the baſe or ground on which they have been depoſited, and partly by their depoſition having been a ſpecies of cryſtallization. The primitive ſtrata, which are generally vertical or highly inclined, had firſt been depoſited from the original fluid in which their ſubſtance was diſſolved, and they had taken the form in which we find them, by having been haſtily cryſtallized. From theſe immenſe maſſes the water is ſuppoſed to have retired into the more hollow parts of the globe, or into caverns in its central parts. This retreat was ſo gradual as to have continued for ſeveral ages, and during its continuance, the ſecondary ſtrata appear to have been formed by depoſition of matter from the ocean,

on the ſides of the vertical and highly inclined ſtrata already formed: and laſtly, by the continued retreat of the ſea, theſe ſecondary ſtrata have likewiſe been left expoſed.

It is objected to this account of the formation of the ſtrata, that it does not account for their inclined or vertical poſition. If matter, it is ſaid, was depoſited from a fluid, it would arrange itſelf in a horizontal bed; the inclination of the bottom or ground on which it is depoſited might have ſome effect in modifying the poſition, but this would ſoon ceaſe; the matter precipitated, from this inclination, muſt be depoſited unequally; being carried forward where the inclination is greateſt, it would tend to arrange the whole horizontally, and the different beds or ſtrata, as they are formed, would approach more and more to that poſition. To what cauſe then are to be aſcribed the vertical, or the highly inclined poſitions which they frequently have, and the parallelism which they preſerve with great exactneſs under very extenſive and varied incurvations?

The force of this objection depends entirely on the ſuppoſition, that in the depoſition of theſe ſtrata, their matter had been previouſly merely ſuſpended mechanically in a fluid, and had ſubſided by reſt. Had this been the caſe they muſt have been arranged in horizontal beds, and ought

of courſe to be found in that poſition. The opinion maintained in the Neptunian theory however is, that they had been chemically diſſolved, and had ſeparated and concreted by a ſpecies of cryſtallization. Theſe cryſtalline depoſites would be in large irregular maſſes, as granite, the rock of primary formation, is; and the fluid ſtill continuing to depoſite matter by cryſtallization, this matter, in conformity to the laws of that proceſs, would cryſtallize on the ſides of the maſſes already produced; and thus the appearance of vertical ſtrata would be formed: or the diviſion of theſe might even be determined by the proceſs of cryſtallization itſelf. With regard to the ſinuoſities and incurvations in theſe ſtrata, it may be difficult to point out preciſely how cryſtallization ſhould produce them. But we have ſufficient evidence from facts to prove that this proceſs is capable of producing ſuch effects, and actually does produce them in caſes ſo obvious that there is no room for doubt. Sauſſure has obſerved, with a view to the illuſtration of this opinion, which he maintains, that theſe very appearances are found in ſtalactites and alabaſter. Theſe conſiſt of different layers, which, inſtead of being in a ſtraight line, are found with all theſe varieties of bending and waving which in the ſtrata are conſpicuous on a large ſcale. For the production of theſe no juſt cauſe

can be aſſigned but the cryſtallization, imperfect as it is, by which theſe ſubſtances are formed, a cauſe by which not only the figure, as Dr. Hutton has alleged, but likewiſe the ſtructure of bodies, is determined. It is obvious, that ſince this is proved to be a ſufficient cauſe, the reaſoning is nugatory which is founded merely on the difficulty of conceiving *a priori* how it ſhould produce thoſe effects, becauſe the anſwer is ſufficient that it actually does ſo. And although we may be unable to ſay how cryſtallization ſhould give riſe to "the inflexion of the "ſtrata, the ſimple curvature which they affect, "and that paralleliſm of their layers which in "all their bendings is ſo accurately preſerved*," yet we ſee in the example quoted by Sauſſure that it can produce all theſe appearances, and therefore it may, on a larger ſcale, be adequate to their production in the mountains of the globe. The difference of magnitude cannot alter the nature or powers of the proceſs employed.

In reality, ſome cauſe of this kind muſt even be had recourſe to by the Huttonian geologiſt. It is ſuppoſed indeed that the ſtratification has been the effect of depoſition from water; but this cannot be admitted; for although the materials of theſe ſtrata might at their depoſition be

* Illuſtrations, p. 232.

conceived in the Huttonian theory to be arranged in horizontal beds, yet their ſubſequent fuſion, which in theſe primary rocks is ſuppoſed not to have been partial, but complete or nearly ſo, muſt have obliterated this original ſtratification; and the diviſions they actually exhibit can ſcarcely be accounted for but on the ſuppoſition that they received them in the proceſs by which they conſolidated. The thinneſs of theſe bent layers too, in the greater number of rocks, in micaceous ſhiſtus for example, favours this opinion: it is ſuch, that we cannot conſider the diviſions of them as analogous to the diviſions of beds or ſtrata depoſited in a horizontal poſition from water, and afterwards elevated. And the very forms of theſe contortions are too complicated to admit of the ſuppoſition that they have been produced by an expanſive power applied to theſe rocks while ſoft.

The poſitions of the ſecondary ſtrata may be aſcribed partly to the ſame cauſe, and partly to the direction of the baſe on which they recline. It is certain, that cryſtallization always commences from the ſolid ſurface in contact with the fluid; to this the ſolid maſs adheres, taking of courſe more or leſs perfectly its figure or poſition. In this manner is it conceived by Werner, that the poſitions of the inclined ſtrata have been determined; they have been depoſited by

an imperfect cryſtallization, mingled ſometimes with a mechanical ſubſidence, and have adhered to the ſides of the primitive ſtrata, on which they are incumbent. And, from the ſame cauſe, any bending which they have, different from that of the baſe on which they reſt, may be explained. Hence we find, in conformity to this theory, that the ſecondary ſtrata are more inclined in the neighbourhood of the primitive vertical mountains; and, as they recede from theſe, are more nearly horizontal; and theſe alſo are more horizontal, in proportion as they are mechanical depoſites.

Another circumſtance may be pointed out, as having probably ſome influence on the poſition of certain ſtrata. It has been ſuppoſed, and the ſuppoſition is admitted by Profeſſor Playfair not to be improbable, that, after having been depoſited horizontally, ſome ſtrata may have ſunk unequally, from the unequal diſtribution of their weight. A degree of inclination would thus be given to the whole, more or leſs, according to the degree of unequal depreſſion that may have taken place. Werner has obſerved, that ſuch ſinkings of the ſtrata at their formation are not improbable, and they fully account for the inclined poſitions of various ſtrata which have not derived their arrangement from cryſtallization.

Or if it ſhould even be ſuppoſed, that the poſitions of the ſtrata indicate the exertion of an expanſive power from beneath, by which they have been elevated, ſuch a cauſe, it may be remarked, may be admitted into the Neptunian ſyſtem as not inconſiſtent with its principles. The general appearance of theſe ſtrata prove, that, at the period of their formation, they had been ſubject to the moſt important revolutions. They have been disjoined from each other, the beds of rivers hollowed out, iſlands ſeparated from the mainland; and, in a word, have ſuffered every ſpecies of fracture and diſlocation. No cauſe can be aſſigned for theſe effects, more probable perhaps than that of an expanſion produced from an accumulation of heat in the interior parts of the globe, the power of which has probably been ſpent in their production. No appearances in the elevation of theſe ſtrata prove the peculiar tenets of the Huttonian theory, that the ſtrata thus elevated had once been fuſed, or that a ſubterraneous heat has always acted, and ſtill continues undiminiſhed. On the contrary, it has been demonſtrated, that theſe propoſitions are falſe, as being utterly inconſiſtent with the known laws of heat; and it will immediately be ſhown, that, from ſuch a cauſe, the original horizontality of theſe ſtrata cannot poſſibly be explained. Were it there-

fore clearly proved that they had been elevated by an expanſive power, we muſt adopt ſome ſuch ſuppoſition as that now ſtated, that it had been by a cauſe acting at their formation, either the accumulation of heat, the diſengagement of it by the proceſs of cryſtallization itſelf, or the extrication of aerial fluids; a ſuppoſition which ſufficiently accords with the other principles of the Neptunian theory, and is probable in itſelf. One of this kind has even occurred to a celebrated Neptuniſt, Sauſſure. He obſerves, that after the materials were depoſited, and formed horizontal beds, it may be imagined that "heat, or elaſtic fluids ſhut up in "the interior parts of the globe, had raiſed up "and broken the external cruſt, had thus ex-"truded the interior and primary part of that "cruſt, while the external and ſecondary parts "remained ſupported on the others. Into the "cavities formed by ſuch eruptions the waters "would ſink, and leave the elevated land *." Such a ſyſtem is ſimilar to that firſt ſketched by Lazarus Moro, and is a modification of the Neptunian theory that may be maintained with much advantage. It is doubtful, however, whether ſuch an expanſive power is to be admitted as a general cauſe, or whether, if it has exiſted at all, it has not been local in its exertion, and of courſe the cauſe of particular appearances.

* Voyages aux Alpes, t. iv. xcı.

We have next to conſider the account which the Huttonian theory gives of the arrangement of the ſtrata. In this firſt, and perhaps moſt important part of the proof from induction, it will be found ſingularly defective.

It is ſuppoſed that the materials of theſe ſtrata have been depoſited from water in a looſe or divided ſtate. From ſuch a depoſition it is not at all difficult to conceive that beds or layers, parallel to each other, might be formed: And if it were admitted that theſe materials could be conſolidated merely from the exertion of the power of aggregation between their particles, their original arrangement might be preſerved, and their ſubſequent elevation by an expanſive power would produce the general appearances which the ſtrata actually exhibit. But in the Huttonian ſyſtem it is maintained, that no conſolidation can ariſe from ſuch a cauſe; it can be accompliſhed only by fuſion, either partial or complete. Now, by ſuch a fuſion, it is obvious that the original diviſion of the depoſites into beds or ſtrata, muſt be completely ſubverted; for each is rendered ſo ſoft or fluid as to become homogeneous, and capable of yielding to preſſure; and no cauſe is pointed out how this fluid or ſoftened maſs is again to be formed into diſtinct ſtrata. This important objection may be illuſtrated by a few examples.

Sandſtone is found in beds or ſtrata of very different degrees of thickneſs, generally horizontal in their poſition, or ſomewhat inclined. Theſe are ſuppoſed by the Huttonian geologiſt to have been formed from matter depoſited from the ocean, and ſoftened by ſubterraneous heat. But let him explain by what cauſe in their ſubſequent conſolidation they have been formed into diſtinct ſtrata. It may perhaps be ſaid to ariſe from the contraction of the maſs in cooling; and this appears to be the ſolution which the author of the theory gives: "The "contraction of the maſs conſolidated by fuſion, "or the effect of fire, is the cauſe of thoſe na-"tural diviſions in the ſtrata*." But why, it may be aſked, are the rents horizontal? the direction of all others the moſt unfavourable, from the gravity of the maſs; and why do they preſerve this direction with ſo much regularity to ſo great an extent? Theſe circumſtances clearly prove that the parallelism of theſe ſtrata is that which they originally had from their depoſition from water. Between the different ſtrata there are alſo often interpoſed thin layers of earth or clay, a proof that they had been formed ſucceſſively, and that on each of them, ſubſequent to its formation, this earthy matter had been

* Theory of the Earth, vol. ii. p. 56.

depoſited; that the whole had not been reduced into a ſoft or liquid maſs, which on cooling had ſplit into horizontal beds, but that the original ſtratification had been preſerved.

This is even admitted by Profeſſor Playfair: "In beds of ſandſtone nothing is more frequent "than to ſee the thin layers of ſand ſeparated "from one another by layers ſtill finer of coal- "ly or micaceous matter, that are almoſt ex- "actly parallel, and continue ſo to a great ex- "tent, without any ſenſible diminution. Theſe "planes can have acquired their paralleliſm on- "ly in conſequence of the property of water, "by which it renders the ſurfaces of the layers "which it depoſites, parallel to its own ſurface, "and therefore parallel to one another *." But if this be admitted, the difficulty occurs in its original force. If the ſandſtone has been conſolidated by fuſion ſubſequent to its arrangement, how have theſe diviſions remained diſtinct, and preſerved their exact paralleliſm? and how has the interpoſed clay eſcaped conſolidation?

Preciſely the ſame difficulty occurs in the primitive ſtrata, which, though now vertical, have, according to the Huttonian geologiſt, been originally horizontal. No particular fact could be ſelected more forcible in illuſtrating this dif-

* Illuſtrations, &c. p. 44.

ficulty, than one ſtated by Mr. Playfair: "In "the micaceous and talcoſe ſhiſti, thin layers "of ſand are often found interpoſed between "the layers of mica or talc. I have a ſpecimen "from the ſummit of one of the higheſt of the "Grampian mountains, where the thin plates "of a talcy or aſbeſtine ſubſtance are ſeparated "by layers of a very fine quartzy ſand not "much conſolidated*." Theſe kinds of ſhiſti having more or leſs of a cryſtalline texture, muſt have been nearly, if not completely, fuſed, as is indeed allowed, and even ſuppoſed. How then have they remained divided into thin layers, the paralleliſm of which is preſerved with more accuracy, and to a greater extent, than even in the ſecondary ſtrata? Or how has the ſand, interpoſed between theſe layers, eſcaped perfect conſolidation? If it were equally fuſible with the ſhiſtus, or nearly ſo, it is obvious that it ought to have been vitrified or aglutinated, and of courſe perfectly conſolidated. If it were leſs fuſible, it is not leſs evident that it ought to have been imbedded or cemented in the fuſed ſhiſtus. Theſe ſtrata are ſuppoſed to have been originally horizontal. Suppoſe the firſt or undermoſt ſtratum conſolidated, over this has been depoſited a layer of ſand; the forma-

* Illuſtrations, &c. p. 167.

tion of another ſtratum of ſhiſtus by fuſion, took place above this: But it is evident that the matter of this ſtratum, fuſed or ſoftened, muſt by its mere weight have enveloped the particles of ſand beneath it; and when we reflect on the ſucceſſion of theſe layers, the difficulty is much increaſed: the heat exerted on the undermoſt layers muſt have been intenſe, and the preſſure upon theſe at the ſame time great. On no hypotheſis, therefore, agreeable to the Huttonian theory, could ſand, looſe or little conſolidated, be found between them, nor can the diviſion of theſe layers themſelves be explained.

If we conſider this difficulty with regard to ſtrata of different kinds alternating with each other, the argument will be found to gain, if poſſible, in force. By what cauſe have the diviſions between theſe different ſtrata been produced? The ſuppoſition of ſucceſſive formation, which in the Neptunian theory fully accounts for ſuch an arrangement, can in the Huttonian have no place. Let us ſuppoſe matter to be depoſited, fuſed or ſoftened, and conſolidated, how is another ſtratum of a different kind of matter to be formed above it? Materials for it might be collected, but theſe, according to the theory, cannot be conſolidated without heat. The heat neceſſary for this purpoſe being applied from beneath, muſt operate with ſtill more force on the

ſtratum on which theſe looſe materials lie, and in innumerable caſes muſt completely fuſe it, ſince in the various poſitions and alternations of the ſtrata, the leſs fuſible are often placed over thoſe which are more eaſily fuſed. There is no proviſion therefore made in this ſyſtem for the formation of a number of ſtrata ſuperincumbent on each other. The upper ſtratum cannot have been laſt formed, becauſe the heat neceſſary for its conſolidation muſt have fuſed theſe beneath, and the materials of it would of courſe ſink, and be imbedded in the fuſed matter, before they could be conſolidated: The under ſtratum cannot poſſibly have been formed, and raiſed ſo as to be applied preciſely to the one above it, and this through an extenſive ſeries: And laſtly, the formation cannot be ſuppoſed to be ſimultaneous, for no cauſe can be given why by the neceſſary degree of heat the whole ſhould not have been homologated, or how they ſhould afterwards have ſeparated into diſtinct horizontal ſtrata of the greateſt extent, of different materials, and of various degrees of fuſibility and ſtates of induration.

In examining the actual poſitions and connections of the ſtrata, theſe difficulties might be abundantly illuſtrated; but to avoid a tedious diſcuſſion, it may be ſufficient to notice only a

few of theſe in which they are placed in the cleareſt light.

No alternation is more frequent that that of calcareous and argillaceous ſtrata. To account for their conſolidation, the Huttonian ſuppoſes them to have been in a fuſed or ſoftened ſtate; but if they ever were in ſuch a ſtate, they muſt, from the attraction between the earths compoſing them, have combined, and an entire homogeneous maſs, not a number of diſtinct alternating ſtrata, muſt have been formed; or at leaſt where they were contiguous, they muſt have united, and every trace of the original ſtratification have been obliterated: yet ſo far from there being any union of this kind, the line of ſeparation is perfectly diſtinct, frequently ſo much ſo that they are not in firm adheſion.

Another alternation extremely common is that of limeſtone with argillite, the alternation being continued through an extenſive ſeries. One of theſe muſt be leſs fuſible than the other; and as they alternate, it is a matter of perfect indifference in the argument which of them is ſuppoſed to be ſo. Let it be ſuppoſed, as is probably the fact, that the limeſtone is the leaſt fuſible: A ſtratum of it may be conſidered as commencing the ſeries: on it the materials of the argillite have been depoſited from the ocean, and as theſe are ſuppoſed to be more fuſible

than the limeſtone, it is poſſible that the central heat may have operated through the latter, without fuſing it, and have conſolidated the former. But above this ſtratum of argillite is another ſtratum of limeſtone. How could it have been conſolidated? By the ſuppoſition made, the argillite is more fuſible; the central heat could not therefore operate through it, ſo as to conſolidate the materials of the ſuperior limeſtone without fuſing it, and from this fuſion the materials of the limeſtone ought neceſſarily to have been imbedded in the argillite. And when we conſider that an alternation of this kind is often found in an extenſive ſeries, by which both an intenſe heat and a great preſſure muſt have been exerted on the lower parts, it is obviouſly impoſſible that theſe different ſtrata could have been conſolidated by a central heat, ſo as to be kept diſtinct.

Strata of rock-ſalt are ſometimes covered by ſtrata of ſandſtone, or limeſtone. The Huttonian geologiſt muſt ſuppoſe that this ſandſtone has been conſolidated by the central heat acting through the rock-ſalt below it. But this is plainly an impoſſibility. The ſalt is a ſubſtance comparatively very fuſible, as it can even be volatilized by the heat of a coarſe pottery furnace, while ſandſtone is very infuſible. The heat neceſſary therefore to ſoften ſandſtone, in this

poſition, muſt have melted the ſalt beneath; and as this latter ſubſtance is of a much inferior ſpecific gravity, the ſandſtone muſt have ſunk in it, and the arrangement obſerved in nature could never have been produced.

Laſtly, we find in innumerable caſes ſtrata more imperfectly conſolidated than others above them, and of courſe farther removed from the conſolidating power, though the difference cannot be aſcribed to any difference in the fuſibility of the ſubſtances compoſing them. An example will place this in a clear light. In a ſection of the ſtrata at Newcaſtle, coal is found at the depth of 102 feet. Over it is a bed of black clay 13 feet thick, with impreſſions of ferns in its ſubſtance; above this, another bed of harder clay 26 feet in thickneſs. The ſtratum incumbent on this is a hard quartoſe ſandſtone, with ſpecks of mica, 25 feet thick; and this is again covered by clay. Now how could this ſandſtone have been conſolidated by the ſubterranean heat, while ſo many feet of clay beneath it, and of courſe nearer the operation of that heat, had not even been indurated? We may pronounce it impoſſible that it ſhould be ſo. Nor is the example uncommon: there are many ſimilar to it, and even leſs favourable, as the banks of clay extend to 80, 100, or more fathoms, in thickneſs, with perfectly conſolidated ſandſtone above;

and this is diverſified with alternations of limeſtone, gypſum, coal, and a great variety of other ſecondary ſtrata. It is not poſſible to conceive an arrangement which more clearly indicates their origin, that theſe are ſucceſſive depoſitions from water, varying in their conſolidation, from the different forces of aggregation exerted between the particles of each; and that they have ſuffered no other change to ſubvert or modify their original ſtratification. Could the Huttonian geologiſts bring forward facts ſo forcible againſt the Neptunian theory, it might juſtify the triumphant tone in which they have ſometimes conducted the controverſy.

The truth is, that the Huttonian theory has in this part tacitly aſſumed the explanation of the Neptunian ſyſtem,—that ſtratification is the effect of ſucceſſive depoſition from water. Nor has it been obſerved, that the aſſumption is obviouſly incompatible with its principles; ſince by the ſubſequent fuſion, the original arrangement muſt have been deſtroyed. The actual poſitions of the ſtrata are alſo totally irreconcilable with the notion of their conſolidation having been effected by fuſion, as is evident from the inſtances pointed out; the Huttonian ſyſtem therefore has the ſingular infelicity of failing to account for the firſt and moſt important of all the facts which are the ſubject of a theory of the

earth,—the general ſtratification of the materials at the ſurface of the globe.

It remains to conſider the explanation given in the Huttonian ſyſtem, of the appearances of the UNSTRATIFIED ROCKS. Theſe are principally granite, and the different varieties of porphyry, baſalt, trap or whin, foſſils, which, beſides their common character of in general not being ſtratified, graduate into each other.

It has uſually been believed by geologiſts, that the principal rocks of this claſs are the moſt ancient in the globe. Granite, in particular, is conſidered as the baſe on which all others reſt; and therefore as of prior formation to the others. Dr. Hutton holds preciſely the oppoſite opinion, that theſe rocks are of lateſt formation, or poſterior to the ſtrata, whether primary or ſecondary, which are incumbent on them.

The formation of theſe rocks, in the Huttonian ſyſtem, is aſcribed to the eruption of melted matter from the internal parts of the earth, among the ſuperincumbent ſtrata. It is conceived, that, by the ſubterraneous heat, which is always active, an immenſe quantity of matter is kept in a melted ſtate; that where the expanſive power of this heat is increaſed to a certain point, the ſuperincumbent ſtrata are heaved up, and a portion of this melted matter ejected; and that

this, when conſolidated by cooling, forms granite, porphyry, baſalt, or trap, according to the compoſition of the matter which has been in fuſion.

The arguments for the igneous origin of theſe rocks, from their properties, are afterwards to be conſidered: At preſent we are to notice thoſe drawn from their poſitions.

The principal one of this kind, and on which much is ſaid by the defenders of the Huttonian theory, is that of veins of theſe unſtratified rocks, at leaſt of whin and granite, running into the neighbouring ſtrata. It is admitted, that veins are poſterior in formation to the ſtrata in which they are found; and this appearance, therefore, of veins of whin and granite entering theſe ſtrata, prove, that the former have been laſt formed. It is ſuppoſed, that the matter of theſe had been injected, when in a fluid ſtate, into rents and cavities of theſe ſtrata, and when conſolidated, had given riſe to theſe appearances. And, as theſe veins often proceed from immenſe maſſes of whin or granite, or are connected with them, it ſeems to follow, that theſe muſt have had the ſame origin.

Theſe phenomena are explained by the Neptuniſt, from the fact, that granite is of different formations. In the Wernerian theory, granite is conſidered as the rock to which the title of

Primitive moſt ſtrictly belongs, as being prior in formation to gneiſs, micaceous ſhiſtus, and others of the ſame claſs. But, although its poſitions with regard to theſe ſtrata generally indicate it to be ſo, yet in particular inſtances we are compelled to admit that it has been of later formation. Thus it is ſometimes found, that a ſtratum of granite is incumbent on theſe other rocks, or alternates with them, which proves that it muſt have been poſterior to them in formation. Specimens have alſo been found of granite, in which are contained pieces of gneiſs, and even argillite, a demonſtration of the ſame fact. This diverſity in the times of formation of theſe rocks holds indeed with reſpect to them all. When it is ſaid that granite is older than gneiſs, and gneiſs than micaceous ſhiſtus, it is not meant that it is univerſally ſo, but only generally, and not without exceptions, in each order of theſe rocks. This being eſtabliſhed, the phenomena of granite veins penetrating the other ſtrata, are eaſily explained; it belongs to the claſs of facts now ſtated, which prove, that, in ſome caſes, granite is of later formation than the other rocks named Primitive. According to the Wernerian theory, veins were originally fiſſures in rocks or ſtrata, which happened while theſe were ſtill covered with water holding certain matters in ſolution, and were

filled by theſe matters being depoſited or cryſtallized. It is conceived, therefore, that, in this manner, granite may have been introduced into fiſſures of the primitive ſtrata.

Profeſſor Playfair has obſerved, that this diſtinction between granite of early, and that of recent formation, is purely hypothetical. "It "is," ſays he, "a fiction, contrived on purpoſe "to reconcile the fact here mentioned with "the general ſyſtem of aqueous depoſition, and "has no ſupport from any other phenomena *." From the facts above ſtated, it will be obvious, however, that it is rather a direct inference from phenomena; and the reaſoning by which it is deduced is ſtrictly analogous to that by which other geological concluſions are eſtabliſhed. In all veins there are obſerved undoubted marks of different dates of formation, both with reſpect to each other and to the ſtrata; and there are no grounds for ſuppoſing that granite veins are an exception. The aſſumption, that the granite in maſs, and that in veins, are the productions of different periods, is ſupported by other facts than thoſe from which it is deduced; thus, it is obſerved, that in many caſes, the granite in veins is different in its properties from the other. Nay, this point may be determined by an appeal to the authority of Mr Playfair himſelf. He admits, that whinſtone, into which granite

* Illuſtrations, &c. p. 320.

graduates, and which belongs to the ſame claſs of rocks, is of different formations: "Theſe un-
"ſtratified rocks, diſtinguiſhed by the name of
"whinſtone, are not all the work of the ſame
"period; they differ evidently in the date of
"their formation; and it is not unuſual to find
"tabular maſſes of one ſpecies of whin inter-
"ſected by another *." Now, as this is admitted with reſpect to whin, the probability of its being the ſame with reſpect to granite can ſcarcely be denied, ſince, in the Huttonian ſyſtem, theſe two ſpecies of rocks are believed to be formed in preciſely the ſame manner. Not only is there this preſumption, but the very fact which is ſtated by Profeſſor Playfair, as proving the different formations of whin, is true with reſpect alſo to granite. It has often been obſerved, that ſtrata of granite are interſected by veins likewiſe of granite, which are diſtinguiſhable, however, from the rock in which they run, by being of a different colour or grain. All veins are admitted, by every mineralogiſt, to be poſterior in formation to the ſtrata through which they paſs; and, from Mr. Playfair's own reaſoning, which, in this caſe, is perfectly juſt, this fact muſt be admitted as deciſive proof, that theſe granites are not the production of the ſame period.

Granite, therefore, it may be conſidered as

* Illuſtrations, p. 81.

proved, is of different formations; and the admiſſion of this propoſition affords a ſolution of the phenomenon of granite veins, to which there is no objection.

The ſame ſolution may be given of the veins of whin found in the ſtrata; as it is admitted to have been formed at different periods, it is evident that theſe veins may be accounted for from its being of recent formation.

It has been ſtated as an argument in favour of the igneous origin of theſe rocks, that where they come in contact with the ſtrata, or where veins of them penetrate theſe ſtrata, the latter are indurated at the point of contact. This is obſerved particularly in whin, and it is conſidered as a proof of the whin having been introduced in a melted ſtate, and having, by its heat, conſolidated more perfectly the matter of theſe ſtrata. "Whether ſandy or argillaceous, they "are uſually extremely hard and conſolidated; "the former, in particular, loſe their granulated "texture, and are ſometimes converted into "perfect jaſper*."

Theſe, and other alterations which the ſtrata ſuffer, are aſcribed, by Werner, to the action of the ſolvent filling the vein. This will percolate to a certain extent through the neighbouring rock, and, by acting chemically upon it, as

* Illuſtrations, &c. p. 73.

Werner ſuppoſes, or perhaps by depoſiting part of the matter it has in ſolution, may alter its appearance and properties. Thus, in the preſent example, if, into a fiſſure in a ſtratum of ſandſtone, the fluid, holding the matter of trap in ſolution, were introduced, it would percolate to a greater or leſs extent through the ſandſtone, which is generally porous, and the matter depoſited in the ſandſtone by this percolation might give it the hardneſs of jaſper. Jaſper is preciſely ſuch a foſſil as might be expected to originate from an admixture of this kind, as it conſiſts of ſiliceous, with a large proportion of argillaceous, earth.

This explanation, as an hypotheſis, is ſufficiently probable, and the great proof that it is juſt, is, that theſe alterations in the neighbouring rock are frequently ſuch as the Huttonian theoriſt cannot explain, on the ſuppoſition of the matter of the vein having been in a fuſed ſtate, and having acted by its heat. They are not always induration, but often a change preciſely the reverſe. Thus, when veins run through granite or gneiſs, the felſpar is often changed into kaolin or fine clay, as it is by expoſure to air and moiſture; the mica is alſo decompoſed, and in ſiennite the hornblende, ſo that in the latter a green friable earthy matter is produced. "The mountain of Scharfenberg is of red gra-

" nite, very hard, and of an equal grain; it is " traverſed by metallic veins, of which the *gan-* " *gue* is white or reddiſh quartz, calcareous ſpar, " and white clay. The granite is commonly " much decompoſed along the ſides of the veins; " neither its felſpar nor mica are to be found; " theſe two ſubſtances are ſupplied by a grey " clay, and a green earth, of the nature of ſtea- " tite: this decompoſition extends ten or " twelve inches on each ſide of the vein; be- " yond this the granite reſumes its uſual ap- " pearance. Sometimes a ſhiſtoſe rock is chan- " ged into a ſoft clay, which forms a border " ſome inches thick along the ſides of the " vein *."

It is obvious, that theſe are changes preciſely oppoſite to what would have been produced by heat, while they are ſuch as might have originated from the percolation of a fluid, and are thoſe, indeed, which are produced by expoſure to moiſture. If we ſhould, therefore, refer the change produced by veins of whin on the ſtrata of ſandſtone to a different cauſe, we ſhould at once loſe all unity of theory, and relinquiſh a principle capable of affording a ſatisfactory explanation. The appearance in ſandſtone is one which may juſtly be aſcribed to the ſame cauſe that has produced the change in the granite or

* Journal des Mines, No. xviii. 85.

gneiſs; and its particular nature may be aſcribed to the peculiar action of the percolating fluid, and the nature of the ſandſtone itſelf. The contraſt between the two hypotheſes is evident: the Wernerian explains from one principle *all* the alterations in the rocks contiguous to veins; the Huttonian explains only *one ſpecies* of alteration, while there are others not only inexplicable, but inconſiſtent with the ſuppoſition by which that *one* is ſolved.

In ſome caſes alſo it appears that the formation and filling of veins have been nearly ſimultaneous with the conſolidation of the ſtrata; and in ſuch caſes, Werner has remarked that the *gangue* participates conſiderably of the nature of the rock, and the rock in its turn is more impregnated with the matter of the vein. If this has been the caſe with ſome of the veins of whin in the ſtrata, the intermixture, and the induration ariſing from it, might be more complete.

The induration which is obſerved where beds of whin come in contact with ſtrata of ſandſtone, may be explained on the ſame principle; either from the percolation of the fluid loaded with particles of whin, or from a degree of intermixture from a formation nearly ſimultaneous. The ſame explanation may be given of the induration of fragments of ſandſtone ſometimes found

in veins of whin, as theſe are in a ſituation in which this percolation muſt have taken place to the greateſt extent.

Another fact may be added, which proves that this induration muſt in all theſe caſes be aſcribed to ſome ſuch cauſe: It is, that ſandſtone is often highly indurated when in contact with ſubſtances where the cauſe aſſigned by the Huttonian geologiſt,—the application of a ſtrong heat, could not have acted. Sandſtone much indurated, it is obſerved by Mr. Jameſon, is often found covered by clay and wacken*: and it will not be affirmed that clay, by any heat it ever had, could cauſe the induration of the ſtratum of ſandſtone on which it is incumbent, while by the operation pointed out in the preceding explanation, ſuch an effect may have been produced.

Dr. Hutton obſerved that where veins of whin run into ſtrata of coal, the coal contiguous to it is frequently found changed in its properties. "It has loſt its fuſibility, and appears to be reduced nearly to the ſtate of coke or charcoal:" a change which is aſcribed to the melted whin having by its heat expelled the bituminous matter of the coal. The fact does not always correſpond with this obſervation, but is ſometimes

* Nicholſon's Journal, vol. iii.

the reverſe. But where it does occur, it may, in common with other alterations of the ſtrata by veins running through them, be aſcribed to the operation of the fluid by which the vein was filled. This percolating through the coal might be capable of producing changes in its compoſition and properties; it might depoſite a portion of earthy matter which would render the coal leſs inflammable: and when we conſider that a ſimilar cauſe can occaſion the decompoſition of felſpar, mica, and hornblende, there is no improbability in the ſuppoſition that it might carry off, or cauſe the decompoſition of the bituminous part of the coal. To either or both cauſes may be owing the change in the properties of that foſſil.

This fact is important in another point of view, as it may probably lead to a demonſtration of the falſity of the Huttonian hypotheſis. The difference between whin and lava is ſaid to be, that the former has been in fuſion under an immenſe compreſſion, while the latter has been in fuſion at the ſurface; and this difference is ſtated as the cauſe of ſome peculiarities in the one compared with the other. It is given as the cauſe of the lava being more or leſs cellular or porous, while the whin is ſolid, as in the latter the air had been retained, which in the former had eſcaped; and likewiſe as the cauſe of the

whin containing calcareous ſpar, while the lava does not, the compreſſion preſent in the fuſion of the former having prevented the expulſion of the carbonic acid. It is admitted, therefore, that wherever whin has been in fuſion without this compreſſion, it ought to aſſume the preciſe characters of lava; it ought to be more or leſs porous, and ought to contain no carbonate of lime. Now in the fact above ſtated, with reſpect to the coal, it is aſſumed that compreſſion had not been preſent, as the coal had allowed its bituminous matter to be expelled. It will not be contended that compreſſion could be removed from the coal, and preſent on the melted whin, at the diſtance of a few inches. If therefore any carbonate of lime ſhould be found in the whin in ſuch a ſituation, either in its compoſition, or in a ſeparate cryſtallized ſtate, (and the probability is, that it will be found;) or even if the whin ſhould not differ ſomewhat in its appearance from common whin, and approach to lava, we ſhall obtain unanſwerable demonſtration of the falſity of ſome of thoſe ſuppoſitions which conſtitute the Huttonian theory.

Again it is ſtated, that "the diſturbance of "the ſtrata, wherever veins of whinſtone a-"bound, if not a direct proof of the original "fluidity of the whinſtone, is a clear indication

" of the violence with which it was introduced " into its place *."

But that ſuch appearances are not effects neceſſarily produced from the invaſion of the ſtrata by ſtreams of melted whin, and do therefore not indicate ſuch an invaſion, is proved by there being frequently no ſigns of diſturbance whatever accompanying veins either of this rock or of granite. Veins of whin traverſe coal without any diſlocation of the bed of coal; and veins of granite are often unaccompanied by theſe marks of violence which might be expected from its irruption into the neighbouring ſtrata. The abſence of theſe in many caſes affords an argument at leaſt equivalent in force to their preſence in others; and indeed, even where they do occur, it is admitted that they do not prove the original fluidity of theſe maſſes.

In the Neptunian ſyſtem there is no difficulty in explaining all theſe appearances, and ſome of them are even ſtated by Werner as proofs of his theory of veins. According to that theory, the fiſſures in rocks and ſtrata, which, when ſubſequently filled, form veins, have in general ariſen from an unequal ſubſidence of the rock in which they are found, one part of it from a difference in its height, in the materials compoſing it, or in the want of ſupport from contiguous rocks having ſunk more than the other. It is

* Illuſtrations, &c. p. 74.

obvious, therefore, that the rock on the one ſide of the fiſſure produced by ſuch a cauſe, will be lower than the rock on the other, and hence the ſhifting of the ſtrata obſerved by the ſides of veins. Theſe ſinkings of the ſtrata, producing ſuch fiſſures, have alſo taken place at various times, as is proved by ſeveral facts in the appearances of veins, particularly by that of one vein croſſing another; and from this circumſtance the diſturbances ſometimes preſent are eaſily accounted for. If after a vein was formed, a new ſubſidence had taken place in the rock in which it was ſituated, it is evident that diſlocation, both of the vein itſelf, and of the contiguous parts, muſt have taken place to a greater or leſs extent.

It is to be obſerved alſo, as a fact highly favourable to the Neptunian theory of theſe diſturbances, that the fiſſures where theſe ſlips or heavings of the ſtrata take place, are frequently filled with ſubſtances, ſuch as ſoft clay or ſand, which cannot be ſuppoſed to have been ejected with any projectile force, or to have produced any alteration of the exiſting arrangement by the violence of their introduction.

It is further ſtated, that when whin is found interpoſed in tabular maſſes between beds of ſtratified rocks, " it is not uncommon to find " the ſtrata in ſome places contiguous to the

" whin, elevated, and bent, with their concavity " upward, ſo that they appear clearly to have " been acted on by a force that proceeded from " below, at the ſame time they were ſoftened, " and rendered in ſome degree flexible *." An appearance of this kind, however, might equally be produced from the ſubſidence of certain parts of the ſtrata while they were ſoft and yielding, as from the elevation of other parts of them; and conſequently, ſuch an appearance affords no certain indication of the nature of the cauſe that has operated on them.

The laſt argument, from the poſition of whin, is thus ſtated by Profeſſor Playfair: " If it be true, " that the maſſes of whin thus interpoſed among " the ſtrata, were introduced there after the " formation of the latter, we might expect to " find, at leaſt in many inſtances, that the beds " on which the whinſtone reſts, and thoſe by " which it is covered, are exactly alike. If " theſe beds were once contiguous, and have " been only heaved up, and ſeparated, by the " irruption of a fluid maſs of ſubterraneous la- " va, their identity ſhould ſtill be recogniſed. " Now, this is preciſely what is obſerved; it is " known to hold in a vaſt number of inſtances, " and is ſtrikingly exemplified in the rock of " Saliſbury Craig, near Edinburgh.

* Illuſtrations, &c. p. 75.

" The ſimilarity of the ſtrata that cover the " maſſes of whinſtone, to theſe that ſerve as the " baſe on which they reſt, and again, the diſſi- " milarity of both to the interpoſed maſs, are " facts which I think can hardly receive any " explanation on the principles of the Neptu- " nian theory. If theſe rocks, both ſtratified " and unſtratified, are to be regarded as pro- " ductions of the ſea, the circumſtances would " require to be pointed out which have deter- " mined the whinſtone, and the beds that are " all around it, to be ſo extremely unlike in " their ſtructure, though formed at the ſame " time, and in the immediate vicinity of one " another; as alſo thoſe circumſtances on the " other hand, which determined the ſtratified de- " poſites, above and below the whinſtone, to be " preciſely the ſame, though the times of their " formation muſt have been very different. The " homogeneous ſubſtances thus placed at a di- " ſtance, and the heterogeneous brought ſo " cloſely together, are phenomena equally un- " accountable, in a theory that aſcribes their " origin to the operation of the ſame element, " and that neceſſarily dates their formation ac- " cording to the order in which they lie one " above another *."

* Illuſtrations, &c. p. 75.

The argument is here ſtrongly ſtated; but nothing is more eaſy than to ſhow that it is equally concluſive againſt the Huttonian theory; that the difficulty which is urged is a particular example of one more general, which is common to both. In the view which is preſented to us, our attention is confined to the alternation of whin with the ſtratified rocks; but all that is ſaid applies with the ſame force to the alternation of the ſtrata with each other. Theſe, according to both theories, are formed in the ſame manner; and the general difficulty preſents itſelf, how we ſhall account for a ſtratum of a particular kind being formed, for the formation of it being interrupted, while one of a different kind is produced above it; and for the formation of the firſt being again reſumed, ſo that it ſhall be preciſely ſimilar to the one beneath, though it is cut off from all communication with it, by the ſtratum of a different kind which is interpoſed. This will be rendered evident by an illuſtration: Among the vertical ſtrata, micaceous ſhiſtus and ſlate frequently alternate with each other; according to the Huttonian theory, theſe, previous to their elevation, were in a horizontal poſition; the micaceous ſhiſtus, ſuppoſe, is undermoſt, and muſt have therefore been firſt formed; over this is the ſlate; and again, above this, is the micaceous

ſhiſtus, the ſame as beneath. In this caſe, it cannot be ſuppoſed that the ſlate has been introduced in fuſion between the two ſtrata of ſhiſtus, for there is frequently an extenſive ſeries of alternations, and both of theſe rocks are conſidered, even in the Huttonian ſyſtem, as ſtratified, and of the ſame formation. We have here, therefore, the difficulty which Profeſſor Playfair ſo ſtrongly urges, of accounting for the ſimilarity of ſtrata between which another is interpoſed, and the diſſimilitude of thoſe to this interpoſed maſs. In the ſtrata which remain horizontal, the caſe is preciſely the ſame. An alternation which not unfrequently occurs, is that of limeſtone with argillite. The ſtrata of limeſtone, between which the argillite is interpoſed, are alike in their properties; yet it will not be ſuppoſed that theſe ſtrata have been divided, by the argillite having been introduced between them in fuſion. They muſt be admitted to be of ſucceſſive formation; and the argument of the learned Profeſſor may, in this caſe, be directed with its full force againſt his own ſyſtem; for, if they have been formed in the ſame manner, as is admitted, what cauſe can have determined the formation and depoſition of the argillite, between the formation of the ſtrata of limeſtone? and how ſhould theſe latter

be alike, when the times of their formation muſt neceſſarily have been different?

The force of this objection can now be appreciated. If the ſimilarity of ſtrata, ſeparated by interpoſed maſſes of another kind of rock, were confined to that particular caſe where whin is the interpoſed ſubſtance, the argument thence derived in favour of the Huttonian ſyſtem would be of ſome force: But the phenomena is much more general; it is equally obſervable in the alternations of ſtrata, whether primitive or ſecondary, with each other; and the Huttonian ſyſtem can in ſuch caſes afford no explanation more ſatisfactory than the Neptunian, of the ſimilarity exiſting between the ſeparated ſtrata, and the diſſimilarity of theſe to the ſtratum interpoſed between them. It muſt not be forgotten that thoſe inductions, which conſtitute our theories, cannot in every point be complete; and appearances muſt frequently be found which, from our imperfect knowledge, cannot be fully explained. The preſent is one of this kind. Whether we aſcribe the formation of the ſtrata to the ſole agency of water, or to the operation of fire, we ſhall find it equally difficult to aſſign preciſe cauſes for the total difference in nature of contiguous foſſils and rocks, which muſt have been formed at nearly the ſame period, or for the ſeries of alternations which they obſerve with

reſpect to each other. Werner conſiders ſuch facts as proofs that the original or chaotic fluid had, at different periods, held different ſubſtances in ſolution, and that from this had originated the ſucceſſive ſtrata of different kinds. The ſame ſolution too might ſucceſſively afford depoſites of different kinds, according to the affinities that had been exerted having been varied by the very combinations taking place.

This alternation of the ſtrata leads to the conſideration of a point of much importance in geology,—the tranſition of different kinds of rock and ſtrata into each other. In thoſe examples of alternation, which have been the ſubject of the argument now diſcuſſed, the line of ſeparation between the contiguous maſſes is ſuppoſed to be perfectly diſtinct, as it frequently is; but in many other caſes the tranſition is gradual, or the one kind of rock paſſes inſenſibly into the other; and where this tranſition takes place from rocks of the unſtratified into thoſe of the ſtratified kind, it is a phenomenon which overturns the Huttonian hypotheſis of the formation of theſe rocks.

According to that hypotheſis, the ſtratified and unſtratified rocks have been formed at very different periods, and even in different modes. The former have been firſt arranged and conſolidated, and afterwards the matter compoſing

the latter has been thrown up in a fuſed ſtate, and has invaded the others, filling up every cavity. According to this view of the ſubject, it is evident that the line of diſtinction between them ſhould always be well marked. A ſlight alteration might take place in the appearance or hardneſs of the ſtratified rock, from the heat of the melted matter brought into contact with it; but there ſhould be no ſuch thing as an imperceptible gradation or tranſition of the one into the other, while, if both are formed in the ſame manner as the Neptunian theory ſuppoſes, ſuch a gradation is not more than what might be expected occaſionally to take place.

This tranſition, therefore, of the unſtratified into the ſtratified rocks being ſo unfavourable to the Huttonian hypotheſis, is attempted to be denied: "If, in theſe inſtances, (ſays Mr. "Playfair) the gradation were inſenſible, as ſome "have aſſerted it to be, between the ſtrata and "the interpoſed maſs, ſo that it was impoſſible to "point out the line where the one ended and the "other began, whatever difficulties we might "perceive in the Neptunian theory, we ſhould "find it hard to ſubſtitute a better in its room. "But the truth ſeems to be, that, in the caſes we "are now treating of, no ſuch gradation exiſts; "and that, though where the two kinds of rock "come into contact a change is often obſerved,

" by the ſtrata having acquired an additional de-
" gree of induration, yet the line of ſeparation
" is well defined, and can be preciſely aſcertain-
" ed. This is at leaſt certain, that innumerable
" ſpecimens, exhibiting ſuch lines of ſeparation,
" are to be met with; and wherever care has
" been taken to obtain a freſh fracture of the
" ſtone, and to remove the effects of accidental
" cauſes even where the two rocks are moſt
" firmly united, and moſt cloſely aſſimilated, I
" am perſuaded that no uncertainty has ever re-
" mained as to the line of their ſeparation. For
" theſe reaſons, it ſeems probable that the gra-
" dual tranſition of baſaltes into the adjoining
" ſtrata, is in all caſes imaginary, and is, in
" truth, a mere illuſion, proceeding from haſty
" and inaccurate obſervation *."

The obſervations, however, by which the tranſition of the unſtratified into the ſtratified rocks are eſtabliſhed, reſt on authorities too unexceptionable to be overturned by an aſſertion of this kind. They are eſtabliſhed on the authority of Werner, allowed to be the moſt ſkilful mineralogiſt of his age, and whoſe obſervations, becauſe they run counter to the views of the theoriſt, are not to be termed haſty and inaccurate, or mere illuſions; and they are ſup-

* Illuſtrations, &c. p. 44.

ported by the authorities of Sauſſure, Charpentier, Reuſs, and other geologiſts of the higheſt character. Profeſſor Playfair indeed, in a great meaſure, recalls, in a ſubſequent part of his work, the aſſertion that has been quoted. "I "am diſpoſed," ſays he, "to make ſome limita- "tion to what is ſaid in § 72, where I have ex- "preſſed an abſolute incredulity as to ſuch tran- "ſitions as are here referred to. The great ſkill "and experience of the mineralogiſt who has "deſcribed the ſtrata at Scheibenberg, do not "allow us to doubt of his exactneſs, though "ſome of the appearances are ſuch as decom- "poſition and wearing might well enough be "allowed be ſuppoſed to produce." The manner in which this acknowledgment is expreſſed ſufficiently ſhows the reluctance with which the fact is conceded. It is indeed one which bears with the utmoſt force againſt the Huttonian hypotheſis of the unſtratified rocks, and which is but feebly obviated by an improbable ſuppoſition.

The obſervations of Werner eſtabliſhing this tranſition of baſalt into ſtratified rocks, are of the firſt importance, and his reaſoning from them convincing and juſt. At the bottom of the hill of Scheibenberg, he obſerves, "there is firſt a "thick bank of quartzy ſand; above that, a bed "of clay; then a bed of the argillaceous ſtone

" named wacken; and upon this lay the baſalt.
" When I ſaw the three firſt beds running almoſt
" horizontally under the baſalt, and forming its
" baſe, the ſand becoming more fine above;
" then argillaceous, and at length changing in-
" to real clay; as the clay was converted into
" wacken in the inferior part; and finally the
" wacken into baſalt;—in a word, when I found
" a perfect tranſition of pure ſand into argilla-
" cious ſand, of this into ſandy clay, and of the
" ſandy clay, through many gradations, into fat
" clay, wacken, and laſtly baſalt,—I was irre-
" ſiſtibly led to conclude, (as every impartial
" judge, ſtruck with the conſequences of that
" phenomenon, would have been) that the
" baſalt, wacken, clay, and ſand, are of one
" and the ſame formation; they are all the ef-
" fect of a precipitation by the humid way, dur-
" ing one and the ſame ſubmerſion of this coun-
" try; the waters which covered it brought at
" firſt the ſand, afterwards depoſited the argil,
" and changed gradually their precipitation in-
" to wacken, and laſtly into true baſalt."

After again ſtating ſome remarks, to render doubtful, if poſſible, the obſervations of Werner on theſe tranſitions, which ſhew the unwillingneſs with which they are admitted, Profeſſor Playfair propoſes an hypotheſis, to account for them on the Huttonian ſyſtem: " It

" is certain," ſays he, " that the baſis of whin-
" ſtone, or the material out of which it is pre-
" pared by the action of ſubterraneous heat, is
" clay in ſome ſtate or other, and probably in
" that of argillaceous ſhiſtus. It follows of
" conſequence, that argillaceous ſhiſtus may,
" by heat, be converted into whinſtone. When,
" therefore, melted whinſtone has been poured
" over a rock of ſuch ſhiſtus, it may, by its
" heat, have converted a part of that rock in-
" to a ſtone ſimilar to itſelf; and thus may
" now ſeem to be united, by an inſenſible gra-
" dation, with the ſtratum on which it is in-
" cumbent; and phenomena of this kind may
" be expected to have had really happened,
" though but rarely, as a partiular combina-
" tion of circumſtances, ſeems neceſſary to pro-
" duce them*."

The concluſions here follow each other with great rapidity, as if they were obvious and undeniable; yet the whole is a ſeries of hypotheſes brought forward to reconcile, if poſſible, the fact of the gradation of theſe rocks with the Huttonian ſyſtem, but unſupported by any proofs, and even improbable in themſelves. It is not proved, or rendered probable, that whin is formed by fire from argillaceous ſhiſtus. On

* Illuſtrations, p. 285.

the contrary, the compoſition of baſalt is peculiar, as according to the analyſis of it by Dr. Kennedy, it contains a quantity of ſoda, which is comparatively a rare ingredient in the mineral kingdom. Though it were proved, it is not argillaceous ſhiſtus, but unconſolidated clay that the baſalt could have come into contact with in theſe ſtrata; and let Profeſſor Playfair endeavour to eſtabliſh his hypotheſis, by converting clay into baſalt by the application of heat. Nor, laſtly, if the circumſtances were of the moſt favourable kind; if even the ſtratum with which the baſalt was in contact, was actually the material from which baſalt could be formed; the heat of the ſtratum of fuſed baſalt, ſuppoſed to be introduced, could not be ſufficient to convert a large bed of it into wacken. We know that the heat neceſſary to fuſe baſalt is very conſiderable; we obſerve, that at the junctions of whin with the ſtrata, there is often ſcarcely any change, and where any is perceptible, it does not extend above a few inches. What circumſtances, therefore, could have been preſent, which enabled this ſtream of fuſed baſalt to convert, by its heat, a large bed of clay into wacken, into which it imperceptibly graduated. If baſalt, in a ſtate of fuſion, is capable, by its heat, of converting clay into a ſubſtance analagous to itſelf, it is obvious that the ſtrata

which are in contact with it, ſhould always be materially changed; and, above all, if this hypotheſis be true, no ſuch arrangement could poſſibly exiſt, as a bed of unaltered clay or ſhiſtus beneath, and in contact with a ſtratum of baſalt. Yet ſuch connections of theſe ſtrata are not unfrequent. Bergman, for inſtance, ſtates an example of baſalt incumbent on thin beds of clay or bituminous ſhiſtus; and Kirwan, and other mineralogiſts, mention inſtances of trap alternating with argilite. The Huttonian, therefore, is reduced to the dilemma of failing to account, either for the gradation of clay into baſalt, or the contact of this rock with unaltered clay. Laſtly, the general gradation obſerved in this mountain is overlooked. It is not confined merely to that of the clay into the wacken, but extends from the ſand to the baſalt, through the whole ſeries; and it is tranſgreſſing obviouſly the rules of juſt reaſoning, to confine the explanation to one part of it merely, and aſcribe this to a local cauſe, or to refer that part to one cauſe, and the reſt to another.

Neither is this the only example of the gradation of unſtratified into ſtratified rocks. It is alſo obſerved occaſionally in granite; and is indeed a geological truth, of which there can be no doubt. In the Neptunian ſyſtem, as both theſe claſſes of rocks are ſuppoſed to be of ſimi-

lar formation, the phenomenon is not ſurpriſing, but, in the Huttonian hypotheſis, it cannot be explained. Their formation is ſtated to be entirely different. The matter of the unſtratified maſs is ſuppoſed to be thrown up in a fuſed ſtate among the ſtrata; it is applied to them in this ſtate, and may therefore be united to them, but it is in direct contradiction to ſuch a ſyſtem, that the one ſhould inſenſibly graduate into the other. In every hypotheſis, a number of the phenomena to which it relates, will be apparently explained; but if its baſis is not in truth, there will always be ſome which cannot be brought under it, but ſtand in oppoſition to its aſſumptions, proving their fallacy. Such a fact, with regard to the preſent hypotheſis, is the gradation of the unſtratified into the ſtratified rocks; and ſo clear are the indications it affords of the origin of theſe maſſes, that it is with juſtice it has been conſidered as one of the ſtrongeſt proofs of the Wernerian ſyſtem. It gave a deadly blow to the theory of the Vulcaniſts, and it inflicts one not leſs fatal to the Plutonic hypotheſis.

Another fact ſcarcely leſs unfavourable to the Huttonian hypotheſis of the formation of theſe rocks is, that they are not unfrequently, both granite and trap, found ſtratified. The ſtratification of granite, both horizontal and verti-

cal, is eſtabliſhed by the obſervations of Sauſſure, and other mineralogiſts, and is admitted by Mr. Playfair; yet, as granite, according to the ſyſtem he defends, conſiſts of matter in perfect fuſion, thrown up from the central regions, we do not ſee how it could have formed any other than an irregular maſs, and we might with as little reaſon expect to find ſtratification in it as in a bank of lava.

This difficulty is attempted to be removed by the following explanation. "Rocks, of which "the parts are highly cryſtallized, are already "admitted as belonging to the ſtrata, and are "exemplified in marble, gneiſs, and veined gra-"nite. In the two laſt we have not only ſtra-"tification, but a ſhiſtoſe, united with a cryſ-"tallized ſtructure, and the effects of depoſition "by water, and of fluidity by fire, are certain-"ly no where more ſingularly combined. The "ſtratification of theſe ſubſtances is, therefore, "more extraordinary than even that of the moſt "highly cryſtallized granite. Neither the one "nor the other can be explained, but by ſup-"poſing, that while ſuch a degree of fluidity "was produced by heat, as enabled the body "when it cooled to cryſtallize, the whole maſs "was kept in its place by great preſſure, act-"ing on all ſides, ſo that the ſhape was preſerv-"ed as originally given to it by the ſea *."

* Illuſtrations, p. 336.

It is not eaſy to diſcover what is here meant by the ſhape originally given by the ſea being preſerved. Granite is ſuppoſed, in the Huttonian ſyſtem, to be matter which has been completely fuſed in the central regions, and erupted; and if it has undergone theſe operations of perfect fuſion, and of eruption, it cannot ſurely be imagined that it could have preſerved its original ſtratification. And if it is ſuppoſed, that ſtratified granite may not have been formed in this manner, but that its materials have been fuſed in the place where they were depoſited, the unity of the theory is entirely loſt, and two hypotheſes reſpecting the origin of this rock are actually advanced.

It is beſides, impoſſible on any ſuppoſition, to believe that matter completely in fuſion, let it be ſubjected to what preſſure it may, could preſerve its original diviſion into beds. That preſſure would not prevent the gravity of the upper parts of the maſs from being exerted on the under, and if it was fluid, this preſſure muſt have obliterated every trace of ſeparation. The ſtratification of granite, therefore, cannot be explained by the hypotheſis Profeſſor Playfair has advanced.

The ſtratification of trap furniſhes an objection not leſs concluſive, and perhaps more ſtriking, as its ſtrata are generally alternated with

others. Of this a ſtriking example has been ſtated as exiſting at Habichtſwalde, near Caſſel: "On ſecondary limeſtone are found ſtrata of "ſand, clay, wacken, and baſalt, which alter-"nate with each other not leſs than three times, "and always in the ſame order. Over the third "ſtratum of baſalt is found a thick bed of coal, "which is covered by a quartzoſe ſandſtone, "containing the remains of plants, and petrified "wood. Laſtly, on this ſandſtone are found, "in the ſame order as before, ſtrata of clay, "wacken, and baſalt; in the ſand are found "marine ſhells; the ſtrata of baſalt have a baſ-"altic *tufa* intermixed with them, containing "fragments of baſalt, olivin, and vegetable re-"mains *." Reuſs and Dolomieu have alſo obſerved ſtrata of baſalt, alternating with ſtrata of limeſtone.

The argument which this diſpoſition of trap furniſhes againſt the Huttonian theory, of its formation, is very evident. It is ſuppoſed to have been thrown up among the ſtrata, and to have filled any cavities or ſpaces between them. In this manner, it might be conceived, that a rock, or mountain of trap, might be formed. But, is it poſſible that it could have formed ſtrata alternating with others? What are the ſuppoſitions neceſſary to account for ſuch an ar-

* Traité. de Mineralogie par Brochant, tom. ii. p. 609.

rangement as that ſtated above?—that a ſtratum of limeſtone ſhould have exiſted, then a bed of clay, another of ſand, and over this an empty ſpace, correſponding in ſize and direction to the ſtrata beneath; that this arrangement ſhould be repeated three times; and that theſe empty ſpaces ſhould have afterwards been filled by fuſed baſalt and wacken injected into them. It may at once be affirmed, that ſuch ſuppoſitions are inadmiſſible; and that were they made, their extravagance would afford a ſufficient refutation of the ſyſtem that required them. If it ſhould be ſaid, that, in ſuch caſes where trap is ſtratified, it is formed in the ſame manner as the other ſtrata, by its materials having been depoſited, and ſoftened or fuſed; this is at leaſt relinquiſhing the ſimplicity of the theory, and propoſing two modes of formation of this rock. And even this ſacrifice would prove inadequate, for the above arrangement in which *ſand* and *clay* alternate with the others, completely excludes the operation of fire, and proves that the whole muſt have been formed by the agency of water.

Laſtly, the remains and impreſſions of organic ſubſtances found in trap demonſtrate its aqueous origin. In this country, in which this rock is abundant, ſuch appearances are undoubtedly rare, and indeed have not been obſerved. But in other countries, they appear to be far from

being uncommon. Werner found in a vein of wacken, at a depth of not leſs than 150 fathoms, trees, with the branches, and even leaves, petrified; and it is ſtated, on the ſame authority, that different rocks of the trap kind, contain marine ſhells, and even bones of quadrupeds. Nothing can be more obvious, then that ſuch ſubſtances could never have been contained in a ſtream of melted matter thrown from the central regions: or if they had even fallen into it after its irruption, and while ſtill fluid, as it is ſuppoſed may have been the caſe, they muſt have been deſtroyed by the fuſed matter; and indeed the circumſtances, in ſome of theſe caſes, are irreconcilable with ſuch a ſuppoſition.

From this review of the Huttonian theory of the unſtratified rocks, it muſt be evident, that it is attempted to be ſupported by appearances which admit of an equal, and in ſome caſes a more ſatisfactory explanation, from the oppoſite opinion. The occaſional ſtratification of theſe rocks, their gradations into thoſe which are ſtratified, and their alternations with other ſtrata, are inconſiſtent with that opinion, and indubitably prove their aqueous origin.

Under this claſs of arguments, may be conſidered thoſe drawn from the appearances of veins, the ſubſtances filling their cavities falling

properly under the deſcription of unſtratified minerals.

Veins are accurately defined by Profeſſor Playfair, " ſeparations in the continuity of a " rock, of a determinate width, but extending " indefinitely in length and depth, and filled " with mineral ſubſtances different from the " rock itſelf*." It is admitted by mineralogiſts, that theſe veins are ſubſequent in formation to the rocks or ſtrata in which they are found; and the cryſtallized ſtate of the ſubſtances with which they are commonly filled demonſtrates that theſe had been introduced in a fluid form.

The Huttonian theory of veins is, that they have been formed by injection, that the matter filling them has been thrown up in a ſtate of fuſion, and that this fuſed matter entering the rents and cavities of the ſtrata, has conſolidated and produced mineral veins.

According to the Wernerian theory, the cavities of veins have been originally fiſſures in the ſtrata or rocks, produced while theſe were yet ſoft and covered by the waters, by various cauſes, principally by the unequal ſinkings of theſe maſſes at their conſolidation, occaſioned by the various denſities of the ſubſtances com-

* Illuſtrations, p. 57.

poſing them, and their unequal elevations, and by the diminution of the waters by which the mechanical ſupport afforded to the mountain at its ſides was withdrawn. The appearances of veins are preciſely ſimilar to what might be expected to be the appearances of fiſſures produced from ſuch cauſes, as, in their taking a direction approaching more to the vertical than horizontal, in their proceeding without much inflection, in the diameter of them diminiſhing after they have proceeded a certain extent, and in their at length diſappearing entirely, by the approach of their ſides, while they generally continue open to the ſurface, though of a diminiſhed ſize.

Into the fiſſures ariſing from theſe cauſes, the fluid ſtill holding much matter diſſolved, and covering the ſurface, would of courſe find acceſs; and the ſolution in theſe cavities being at reſt, the cryſtallizations from it of the diſſolved matter, would take place with more regularity, and they would be more completely ſeparated from each other, than in the formation of the ſtrata. Many of thoſe veins have alſo been filled by ſucceſſive cryſtallizations, and hence a greater variety of ſubſtances have been introduced into them.

The formation of theſe veins is of very different dates, the fiſſures from which they ariſe having happened at different times. This is evident, from the ſimple fact, that one vein of-

ten croſſes another without interruption or alteration of the matter it contains. A new fiſſure has taken place traverſing the exiſting vein, and has been afterwards filled, forming a new vein, and this, it has been proved by repeated croſſings, has taken place a number of times ſucceſſively. Such repeated fiſſures often derange the direction of the vein, and its relative ſituation with reſpect to the ſides of the rock in which it exiſts.

Several of the arguments in ſupport of the Huttonian theory of mineral veins are drawn from the nature and properties of the ſubſtances they contain, ſuch as their inſolubility in any one menſtruum, and the mutual impreſſions in their cryſtallization. Theſe are afterwards to be noticed; at preſent we are to conſider thoſe deduced from the ſtructure and poſitions of the veins.

That the matter filling the veins has not been introduced in a ſtate of ſolution, is evident, it is alleged, from their being no trace of that ſolvent in the vein, and from the vein itſelf being completely filled up.

But, if the entrance into the vein has been open, as is maintained, theſe appearances are not different from what might be expected. Conſolidation, or cryſtallization, it has already been ſhown, may take place from ſolution, without

any ſenſible portion of the ſolvent being retained; and the matter, conſolidating in an open cavity, would exclude the fluid ſeparated, while if a freſh quantity of the ſolution had acceſs, the cavity might be completely filled up. Or, if the fluid was left, it would penetrate the ſurrounding matter by infiltration, and leave the vein only partially filled.

It is ſaid, "if the veins were filled by depoſition from above, we ought to diſcover in "them ſuch horizontal ſtratification as is the "effect of depoſition from water." The uſual ſtructure of a mineral vein is that of incruſtations, or parallel coats, on its ſides. One ſubſtance, calcareous ſpar, for example, or quartz, adheres immediately to the ſides of the rock, and next to this a maſs of any metallic ore, and theſe may be variouſly intermixed with each other, and even with other ſubſtances. But, if the Neptunian theory were juſt, it is affirmed, that the materials ſhould be diſpoſed in horizontal layers acroſs the vein, inſtead of being parallel to its ſides. "On no ſuppoſition," it is ſaid, "can theſe incruſtations be received as a proof "of aqueous depoſition: It may, indeed, be "certainly inferred from them, that the matter "which they conſiſt of, was fluid at the time of "their formation; but the abſence of all appearance of horizontal diſpoſition in any part

" of the vein, amounts nearly to a demonſtra-
" tion, that this fluidity did not proceed from
" ſolution in a menſtruum." And again, " if,
" as the Neptuniſts maintain, the materials in
" the veins were depoſited by water in the moſt
" perfect tranquillity, it is wonderful that we do
" not find thoſe materials diſpoſed in horizon-
" tal layers acroſs the vein, inſtead of being pa-
" rallel to its ſides; and it ſeems very unac-
" countable, that the common ſtrata, depoſited
" as we are told while the water was in a ſtate
" of great agitation, have ſo rigorouſly obeyed
" the laws of hydroſtatics, and acquired a paral-
" leliſm in the planes of their ſtratification, which
" approaches ſo often to geometrical preciſion;
" while the materials of the veins, in circumſtan-
" ces ſo much more favourable for doing the
" ſame, have done nearly the reverſe, and taken
" a poſition often at right angles to that which
" hydroſtatical principles require. This is a pa-
" radox, which the Neptunian ſyſtem has creat-
" ed, and which therefore it is not very like-
" ly to reſolve *."

This objection it is very eaſy to remove; and it is ſomewhat ſingular that Profeſſor Playfair ſhould have ſtated it in ſuch ſtrong terms, and as even amounting nearly to a demonſtration againſt the Neptunian theory, when the anſwer

* Illuſtrations, &c. p. 251, 253.

is ſo obvious and ſatisfactory. The matter filling veins is perhaps always cryſtallized, or has the cryſtalline ſtructure, ſo as to ſhew that it has been by this ſpecies of conſolidation that it was formed. Now cryſtallization is always promoted by a *nucleus* or ſupport, and in any cavity, invariably takes place from the bottom and ſides. In the caſe of a vein therefore filled with a ſolution of different kinds of matter, the ſubſtance moſt diſpoſed to cryſtallize would firſt form an incruſtation on the ſides and bottom, and afterwards, thoſe which had leſs diſpoſition under the circumſtances of the caſe to cryſtallize. According to theſe circumſtances too, the ſubſtances preſent might be variouſly mixed. Werner ſtates this very fact of the incruſtation of the materials of veins on their ſides as a proof of his theory, and particularly, that the incruſtations on one ſide are always alike and diſpoſed in the ſame order, as on the other ſide of the vein. It is remarked alſo, as highly favourable to the ſame theory, that the different coatings are of a greater thickneſs at a depth in the vein, than they are nearer to the ſurface.

If, on the other hand, the vein had been filled with different kinds of matter in fuſion by injection, it is evident, that theſe ought immediately to begin to arrange themſelves in the order of their ſpecific gravities; the conſolidation

of them from cooling could not commence immediately; and therefore, while ſtill fluid, the heavier ſubſtance, the metal, for example, ſhould fall towards the bottom, and the much lighter matrix riſe to the top, a diſpoſition which is never obſerved: And, when they began to concrete, it is equally obvious that the one leaſt fuſible ſhould firſt become ſolid, while the more fuſible remaining fluid, would be forced aſide and collected apart; and thus there could be none of that intermixture, and thoſe mutual impreſſions, which are generally obſerved in the materials of a vein; nor could the cooling of that part of the matter which was next to the ſides of the vein, (a cauſe ſtated by Mr Playfair), have much effect in preventing this, or in diſturbing the regularity of conſolidation. This hypotheſis, therefore, does not account for the appearances which veins actually exhibit.

It may alſo be obſerved, as an important fact in geology, that veins have occurred in which the depoſitions are horizontal. De Luc gives an example of this kind. The fact is utterly inconſiſtent with the Huttonian theory; while it proves clearly that the vein has been filled by depoſition from above, the depoſite being probably more mechanical than chemical, and therefore taking this form.

It is ſtated as an argument in favour of the

Huttonian account of the formation of veins, that they " contain abundant marks of the " moſt violent and repeated diſturbance," ſhifting of the ſtrata, and ſhifting or heaving of the vein itſelf.

Theſe appearances, however, are likewiſe very eaſily explained by the Wernerian theory: the explanation has indeed been already ſtated, in ſhowing the fallacy of the argument for the igneous origin of whin, from the derangements in the ſtrata which accompany its veins. All veins ariſe from fiſſures in the ſtrata or rocks in which they are ſituated; theſe fiſſures have ariſen from unequal ſinkings of theſe ſtrata, ſoon after their formation, and have taken place ſucceſſively. It is obvious, therefore, that they muſt often be accompanied with thoſe very marks of diſturbance enumerated in the objection, and of courſe, that theſe do not prove veins to have been formed by an irruption of fluid matter from beneath.

Werner has ſtated the principal varieties of diſlocation which attend veins, has ſhown that they admit of the moſt ſatisfactory explanations from his theory, and that the concluſions ſuggeſted by the theory, lead to important practical applications in the art of mining. An order is obſerved even in theſe derangements, which may be connected by a principle; while,

were the Huttonian hypotheſis true, there ſhould be nothing but ruin and diſorder.

" The fact of pieces of rock being found in-
" ſulated in veins, is certainly favourable," it is ſaid, " to the notion of an injected and ponder-
" ous fluid having originally ſuſtained them." Admitting that ſuch pieces reſt in no part on the ſides of the vein, the phenomenon is very eaſily explained, from the fact, proved by various appearances, and admitted in the Huttonian theory, that veins have frequently been filled, not entirely at once, but ſucceſſively; the ſubſtances next to the ſides have been firſt depoſited, and thoſe in the middle of the vein have often been of more recent formation. It is obvious, that if, after a partial incruſtation of the vein, by which its diameter would be diminiſhed, fragments of rock were brought, by the circulation of the ſurrounding fluid, or detached by ſome violent ſinking, or ſubverſion of the rock itſelf, and introduced into the cavity, they might be ſuſtained by the ſides, and the vein being afterwards filled up by new depoſitions, the appearance of an inſulated fragment would be produced. It is alſo poſſible, that after a vein has been filled, and completely conſolidated, a new fiſſure might take place in it, preſerving the ſame direction; and pieces of rock falling into it, from either of the above

cauſes, might be cemented by new matter depoſited from water; and in this manner Werner has explained the origin of cemented fragments of this kind which occur in ſome veins.

A fact in the ſtructure of veins incompatible with the Huttonian hypotheſis, and proving the Wernerian theory, of their having been filled from above, is that of petrifactions, marine ſhells, and even vegetable ſubſtances, being frequently found in the ſubſtances filling the vein, a fact eſtabliſhed on the authority of Werner, and others, and even allowed by Dr. Hutton*, nor can there be imagined any more deciſive. As the matter with which veins are filled is ſuppoſed, by the Huttonian, to be thrown up in perfect fuſion, it cannot be conceived that ſuch ſubſtances ſhould be brought up from the central parts of the globe in this ſtream of melted matter, or that if they had fallen into it, they ſhould have ſuffered no change; while the explanation of their origin is obvious and natural, when we ſuppoſe the matter in which they are incloſed to have been depoſited from the ſea.

An argument of a ſimilar kind may be derived from the fact, that veins are ſometimes filled with ſubſtances which obviouſly could never have been in fuſion. Thus Werner men-

* Theory of the Earth, vol. i. p. 396.

tions a vein at Riegelsdorf, in Heſſe, the materials of which are nothing but ſand and rounded ſtones. And Schreiber cites a vein in a mountain near Allemont, filled with an argillaceous earth and rounded fragments of gneiſs, and which intercepts the metallic veins *. Nothing can more certainly prove, that veins are filled from above, and not formed by irruptions from beneath.

The alteration and decompoſition of the rock at the ſides of the vein, have been already noticed, as being explained more ſatisfactorily by the Neptunian than the Huttonian theory. The decompoſition of granite, hornblende, and gneiſs, into a clay or ſoft earth, for ſeveral inches by the ſide of the vein, cannot be explained, but from the percolation and chemical action of a fluid which has filled the vein.

In the connection of the contents of veins with certain ſtrata, an order is obſerved, inexplicable in the Huttonian theory, but ſatisfactorily explained in the Neptunian. This has been traced by Werner. Tin is never found but in primary ſtrata, principally in granite. Molybdena and Tungſten are found in the ſame ſituations, and of courſe have been formed at the ſame period. Uranium and Biſmuth, though

* Journal des Mines, No. xviii. p. 71.

perhaps of a formation rather leſs ancient, appear never to be found in ſtratified mountains. Gold and Silver are ſometimes found in the latter, though rarely. Mercury, the grey ore of Antimony, and Manganeſe, are ſcovered both in primitive and ſecondary mountains. Copper, Lead, Zinc, and eſpecially Iron, belong to all the ages of the world. Cobalt and Nickel, are generally of recent formation. There is the ſame difference to be obſerved in the ſubſtances which accompany the metals. Felſpar, ſhorl, the topaz, and the beryl, are conſidered by Werner as the moſt ancient. Quartz belongs to all periods. Among the calcareous ſubſtances, the moſt ancient are fluor ſpar, and apatite. Trap is of much more modern formation, and gypſum one of the moſt recent *.

Now it muſt be apparent, that the Huttonian hypotheſis of the origin of veins can furniſh no principle by which any order of this kind can be explained. Theſe materials are ſuppoſed to be thrown from the central regions, in which they had exiſted in a ſtate of fuſion, and their irruption muſt have been merely accidental. No cauſe can poſſibly be imagined why certain metals ſhould have been thrown up only in thoſe rents which were ſituated in the primary rocks; others

* Journal des Mines, No. xviii. p. 90.

in the ſecondary, and a third claſs in ſtrata of both kinds: but in the Neptunian ſyſtem a principle can be diſcovered by which this may have been regulated. The depoſites from the chaotic fluid, it is proved, were ſucceſſive, and it muſt, of courſe, have happened that ſome kinds of matter would be formed and depoſited at one period, others at another, from the play of affinities exerted, and the force with which they were held in ſolution. In other words, the ſame cauſe which determined granite to be firſt formed, may have determined the formation or depoſition of tin, molybdena, and tungſten, at the ſame time; and this principle may be extended to the production of all the others. It is a merit, in a theory, of no trivial importance, that it ſhould thus be able to connect, by one principle, facts of the firſt importance, but apparently ſo difficult to be explained.

Laſtly, there are veins to which the Huttonian hypotheſis cannot poſſibly apply—thoſe which are included in rocks, and ſhut in on all ſides. Theſe cannot be ſuppoſed to have been filled by injection, as the termination of them in the rock is obvious. Profeſſor Playfair is obliged to relinquiſh the general theory, and to ſuppoſe that when "theſe veins are found in ſtratified rocks, ſuch as "have not themſelves been melted, we muſt con-"ceive them to be compoſed of materials more

" fuſible than the ſurrounding rock, ſo that they " have been brought into fuſion by a degree of " heat which the reſt of the rock was able to re- " ſiſt, and on cooling have aſſumed a ſparry " ſtructure. When they are found in rocks of " which the whole has been fluid, they muſt be " conſidered as component parts of that maſs, " which by an elective attraction have united " with one another, and ſeparated themſelves " from the ſubſtances to which they had leſs af- " finity *."

The firſt of theſe explanations, that which regards theſe inſulated veins in ſtratified rocks, may probably afford a proof of the falſity of the general hypotheſis on this ſubject. The matter cryſtallized in theſe veins is generally either quartz, or carbonate of lime. Now, there is no ſubſtance exiſting in the form of a ſtratified rock leſs fuſible than either of theſe foſſils: they conſequently could not have been " brought into " fuſion by a degree of heat which the reſt of " the rock was able to reſiſt;" and therefore the phenomenon of a vein of ſuch ſubſtances in ſtratified rocks cannot be accounted for on the principles of the Huttonian theory.

It is found alſo that theſe inſulated veins ſometimes contain metallic ores; and, indeed, many

* Illuſtrations, &c. p. 259.

metallic veins have been completely worked out; yet Dr. Hutton is pleaſed to tell us that theſe can be derived only from the bowels of the earth. "Look into the ſources of our mi-
"neral treaſures: aſk the miner from whence
"has come the metal into his vein. Not from
"the earth, or air above; not from the ſtrata
"which the vein traverſes; theſe do not contain
"one atom of the minerals now conſidered.—
"There is but one place from whence theſe mi-
"nerals may have come, this is the bowels of
"the earth, the place of power and expanſion,
"the place from whence muſt have proceeded
"that intenſe heat by which looſe materials have
"been conſolidated into rocks, as well as that
"enormous force by which the regular ſtrata
"have been broken and diſplaced *."

"The above (adds Profeſſor Playfair) is a very
"juſt and natural reflection; but if, inſtead of
"interrogating the miner, we conſult the Neptu-
"niſt, we will receive a very different reply. As
"this philoſopher never embarraſſes himſelf
"about preſerving an uniformity in the courſe
"of nature, he will tell us, that though it may be
"time that neither the air, the upper part of the
"earth's ſurface, nor even the ſea, contain at
"preſent any thing like the materials of the

*Theory of the Earth, vol. i. p. 130.

" veins, yet the time was when theſe materials " were all mingled together in the chaotic maſs, " and conſtituted one vaſt fluid, encompaſſing " the earth; from which fluid it was that the " minerals were precipitated, and depoſited in " the clefts and fiſſures of the ſtrata *."

After this declamation, it is unfortunate that the Huttonian ſhould be compelled to admit that there are examples of veins filled with metallic matter, cut off from that ſource from which only, according to Dr Hutton's explanation, they could have been filled, and of which no probable explanation can be given but that by the Neptunian theory. It is even probable that all veins are of this inſulated kind, their limits not being always diſcovered, from their not being explored to ſufficient depth; for it is, *prima facie*, an extreme improbability that rents ſhould paſs through innumerable ſtrata even to the central parts of the globe, and that theſe ſhould be filled by injection to the very ſurface, and through crevices often only a few lines in diameter for a conſiderable length.

An appearance not uncommon in veins is that of their becoming narrower as they deſcend, often leſſening to $\frac{1}{4}$ of an inch or leſs in thickneſs, and either remaining ſo or again becoming wider,

* Illuſtrations, &c. p. 248.

while above, they are perhaps ſome feet in wideneſs. It is obviouſly impoſſible that the matter filling this upper part ſhould have been thrown contrary to its own gravity, by injection, through ſuch a paſſage, from the central parts of the globe.

PART IV.

Of the Support which the Huttonian and Neptunian Theories derive from the Appearances and Properties of Individual Foſſils.

THIS part of the inveſtigation of theſe theories, from induction, is that perhaps from which we may derive the moſt concluſive evidence. The poſitions and connections of the ſtrata, can, in many caſes, be only imperfectly obſerved, and they are alſo often ſuch as might have ariſen from various cauſes. But it is reaſonable to preſume, that in individual minerals properties will be found which ſhall afford undoubted proof whether they have been formed by water or by fire.

In conſidering the examples of this claſs, brought forward in proof of the Huttonian theory, I ſhall take them in the order in which they are ſtated by Profeſſor Playfair. The firſt are thoſe belonging to the SILICEOUS GENUS.

" Foſſil-wood, penetrated by ſiliceous matter,

" is a ſubſtance well known to mineralogiſts;
" it is found in great abundance in various ſi-
" tuations, and frequently in the heart of great
" bodies of rock. On examination, the ſilice-
" ous matter is often obſerved to have pene-
" trated the wood very unequally, ſo that the
" vegetable ſtructure remains in ſome places
" entire; and in other places is loſt in a homo-
" geneous maſs of agate or jaſper. Where
" this happens, it may be remarked, that the
" line which ſeparates theſe two parts is quite
" ſharp and diſtinct, altogether different from
" what muſt have taken place, had the flinty
" matter been introduced into the body of the
" wood, by any fluid in which it was diſſolved,
" as it would then have pervaded the whole,
" if not uniformly, yet with a regular gradation.
" In thoſe ſpecimens of foſſil-wood that are
" partly penetrated by agate, and partly not
" penetrated at all, the ſame ſharpneſs of ter-
" mination may be remarked, and is an ap-
" pearance highly characteriſtic of the fluidity
" produced by fuſion *."

From the appearance of foſſil ſiliceous wood, the unbiaſſed obſerver would be much more ready to infer, that it had been petrified by the operation of water, ſince it is ſcarcely poſſible to believe that it could be ſubjected to the action of fire, and at the ſame time have preſerv-

* Illuſtrations, &c. p. 25.

ed completely the ligneous texture. It may be conceived, that if a piece of wood be immerſed in water, which holds diſſolved a portion of ſilex, as many waters do, the earth may gradually be depoſited in its pores. At the ſame time, from the ſlow putrefaction, or decompoſition, which the wood in ſuch a ſituation muſt ſuffer, its principles may be diſſipated in new products; and if theſe two operations, the depoſition of the ſiliceous matter, and the decompoſition of the wood, bear a certain proportion to each other, the earth will be depoſited in the vacuities left by the vegetable matter; and thus an arrangement will be preſerved ſimilar to that of the ligneous fibre. Hence might ariſe the peculiar character of foſſil ſiliceous wood;—its being entirely deſtitute of vegetabe matter, while it preſerves the texture of the wood, often ſo perfectly, that the particular ſpecies can be diſcovered.

But how ſhall we account for theſe effects, if we are to ſuppoſe the wood to have been ſubjected to the action of melted ſiliceous matter? How could this matter have penetrated the ſubſtance of the wood? Still more, how could it penetrate it, ſo as to preſerve the ligneous texture, and even the delicate reticulated ſtructure? How could the ligneous matter have been removed, while the ſiliceous was depoſited

in its place? The operation of fuſion, by which foſſils are formed, is ſuppoſed, in he Huttonian theory, to take place under a vaſt compreſſion; this would prevent the volatilization or decompoſition of any part of the wood, or if it did not, according to the principles of the ſyſtem itſelf, the wood ſhould have been charred or converted into coal. In ſhort, it ſeems impoſſible to give, it need not be ſaid a ſatisfactory explanation, but any explanation whatever, of the properties of ſiliceous wood by this hypotheſis; on the contrary, were it true, had wood been expoſed to the action of melted flint, it muſt either have been decompoſed and charred by it, or muſt have encruſted it, forming around it a homogeneous indeſtructible maſs.

The particular appearance in ſome ſpecimens of the wood, which has been ſtated as favourable to the Huttonian hypotheſis, that of the line of ſeparation between the part that is petrified and the part that remains unchanged being ſharp and diſtinct, inſtead of affording any preſumption of the ſiliceous matter being introduced in a ſtate of fuſion, is not even explained by that ſuppoſition, for we do not perceive why the fuſed matter ſhould have terminated abruptly, ſo as to preſent this diſtinct line. Were the Huttonian required to explain this circumſtance, he would, in truth, be puzzled for an anſwer,

and, inſtead of affording any ſupport to his opinion, it is an additional difficulty, which he is unable to remove. The appearance ſeems to be owing to the proceſs in the humid way going on very ſlowly, probably from the denſity of the wood, and to its having been ſtopt in thoſe ſpecimens before it was complete. It is conceivable that the external part of the wood may have been ſo completely impregnated with the ſiliceous matter as to prevent the infiltration of the water to the internal parts, and of courſe the proceſs of petrifaction would ceaſe, and this, if the wood has been very denſe, it is poſſible may have proceeded only a ſhort way, and have been abruptly terminated.

There is another fact, with reſpect to ſiliceous wood, which gives indication of its watery origin, while it is inexplicable on the Huttonian hypotheſis; it is, that of ſhells often adhering to, and even indented in the wood, deprived of their calcareous matter, and thus forming ſiliceous petrifactions. The preſence of theſe proves that the wood had been immerſed in water; and the entire converſion of their ſubſtance into ſiliceous matter cannot be accounted for on the ſuppoſition of melted ſilex being applied to them; for by what power was this ſilex to expel the calcareous carbonate, of which they principally conſiſt. On the oppoſite opinion

this admits of explanation, the carbonate of lime, being more ſoluble in water than the ſiliceous earth, would be gradually carried off, and the latter might be depoſited in its place.

In favour of the petrifaction of ſiliceous wood being effected by the medium of water, we have the analogous caſe of the petrifaction of wood in this way by calcareous matter. There are many ſprings, in which a portion of carbonate of lime is held in ſolution, which have the power of petrifying any vegetable ſubſtance thrown into them; and many examples of petrified moſſes and other matters are to be found, which no one can ſuppoſe to be performed by fire.—Here the ultimate effect is the ſame. The only difference is, that in the one caſe the petrifaction is ſiliceous, in the other calcareous. Does not the preſumption follow, that the proceſs by which it has been effected is ſimilar; that in the one, calcareous matter diſſolved in water; in the other, ſiliceous matter in ſolution has been applied to the petrified ſubſtance?

Laſtly, we have demonſtration that ſiliceous petrified wood is formed in the humid way. Mr. Kirwan relates a deciſive proof of this kind: one of the timbers ſupporting Trajan's bridge over the Danube being taken up, and examined, was found to have been converted into agate to the

depth of half an inch, while the inner parts were more ſlightly petrified *.

With ſuch weight of evidence, the concluſion cannot be reſiſted, that ſiliceous wood has been petrified by the medium of water; and this firſt proof of the Huttonian theory, from the properties of minerals, ſerves to eſtabliſh the oppoſite ſyſtem.

This argument with reſpect to the ſiliceous petrifaction of wood, is of more importance than at firſt view may appear. It is not merely in itſelf an example which may be brought in ſupport of the one theory or the other, neither is its importance confined to its proving the ſolubility of ſiliceous earth in water, and the poſſibility of the moſt perfect conſolidation being affected by its depoſition; but it eſtabliſhes a ſimilar formation with regard to other foſſils, and furniſhes a proof capable of being carried to a conſiderable extent. It is obſerved in ſpecimens of wood thus changed, that where there are rents or vacuities in the wood, the ſiliceous matter depoſited in theſe, has always aſſumed the figure and ſtructure of agate. It has the concentric coats of that foſſil, its hardneſs, frequently its various ſhades of colour, and in ſhort, all its properties. If, there-

* Geological Eſſays, p. 140.

fore, the petrified wood is proved to be formed in the humid way, it follows, that agates may be formed in the ſame mode; and this fact again may be puſhed ſtill farther, for agates are almoſt always found incloſed in other rocks, as, for example, in trap, and incloſed in ſuch a manner as to render it undoubted that the rock and the incloſed agate muſt have had the ſame origin. So far, therefore, may the application of this argument be carried, from the intimate connection of theſe foſſils. Dr. Hutton might perceive this connection, and the obligation it laid him under of aſcribing the formation of ſiliceous petrified wood to fuſion, ſince, if he admitted it to have been formed in the humid way, he muſt have been forced to admit, that agates and the rocks in which they are incloſed, might have had the ſame origin. It was, perhaps, a proof of polemical ſkill to aſſume that as an argument which he might have otherwiſe been obliged to obviate as an objection; and this might lead him to maintain, that ſiliceous petrified wood was formed from the introduction of fuſed ſiliceous earth, though its appearance is, *prima facie*, inconſiſtent with that opinion, and though it is fully refuted by facts.

" The round nodules of flint that are found " in chalk, quite inſulated and ſeparate from " one another, afford," it is ſaid, " an argument

" of the ſame kind; ſince the flinty matter, if " it had been carried into the chalk by any ſol- " vent, muſt have been depoſited with a certain " degree of uniformity, and would not now ap- " pear collected into ſeparate maſſes, without " any trace of its exiſtence in the intermediate " parts. On the other hand, if we conceive " the melted flint to have been forcibly injected " among the chalk, and to have penetrated it, " ſomewhat as mercury may, by preſſure, be " made to penetrate through the pores of " wood, it might, on cooling, exhibit the ſame " appearances that the chalk-beds of England " do actually preſent us with *."

This theory of the formation of flint is nearly inconceivable, and is inconſiſtent with the appearances of that foſſil. The kind of injection, by which it is ſuppoſed to be introduced into the chalk, is altogether myſterious; for we perceive not, how, without any ſenſible openings, the flint is to be conveyed into the chalk, or if it were forced in by ſome peculiar exertion of preſſure, how the particles are to be collected, ſo as to form nodules of conſiderable ſize. The arrangement of theſe nodules is alſo incompatible with any notion of this kind. They are not irregularly interſperſed in the

* Illuſtrations, &c. p. 25.

chalk, as they neceſſarily muſt have been, if introduced by any ſpecies of injection, but are found arranged with the greateſt uniformity, in horizontal beds or layers, which are equidiſtant from each other.

That the formation of theſe nodules is owing to the agency of water, is proved by the impreſſions of ſhells on their internal ſurface, or even entire ſhells inhering in the ſubſtance of the flint, completely petrified, and having their calcareous matter entirely abſtracted. It is obvious, that this petrifaction and expulſion of the calcareous matter, while the ſtructure and diviſions of the ſhell often remain unaltered, could never be produced by injection of fuſed ſiliceous matter, while they may, in common with other petrifactions, be eaſily explained from the agency of water. There is a particular ſpecimen of this kind, one of the moſt common, which, perhaps, places this in the cleareſt light,—that of the ſhell of the Echinus, which is frequently found filled with flint, while its texture remains perfectly unaltered; and its calcareous matter is, at the ſame time, ſo completely removed, that it does not effervesce on the application of an acid. This ſhell is, in its natural ſtate, ſo extremely tender, that the leaſt preſſure cruſhes it to pieces; and it has only a ſmall aperture leading to its cavity. Can it be ſuppoſed, that it

has been filled with fuſed flint, by any ſpecies of injection? What would be the circumſtances which muſt have been preſent for ſuch an effect taking place? That the ſhell ſhould have been placed in ſuch a manner that its aperture ſhould lie in the direction of the ſtream of injected fluid, and that it ſhould have ſuch firmneſs, as to ſuffer no alteration from being completely filled with this denſe fluid matter; and were even the ſuppoſition of the exiſtence of theſe circumſtances admitted, the expulſion of the calcareous matter of the ſhell would remain inexplicable. This, and other petrifactions, evidently ſhow, that the particles of flint have been brought together ſlowly, and without violence, by the agency of water. Mr. Kirwan attributes their conſolidation, to the infiltration of that fluid through the ſtrata of chalk *; and their formation into nodules, may be aſcribed, according to the Wernerian hypotheſis, to the particles being collected in cavities formed in the chalk by the extrication of air.

" The ſiliceous pudding-ſtone," it is ſaid, " is " an inſtance cloſely connected with the two " laſt; in it we find both the pebbles, and the " cement which unites them, conſiſting of flint " equally hard and conſolidated; and this cir-

* Geological Eſſays, p. 237.

" cumſtance, for which it is impoſſible to ac-
" count by infiltration, or the infinuation of an
" aqueous ſolvent, is perfectly conſiſtent with
" the ſuppoſition, that a ſtream of melted flint
" has been forcibly injected among a maſs of
" looſe gravel *."

There ſeems nothing improbable in the ſuppoſition that the pebbles in this foſſil have been agglutinated and conſolidated by the depoſition of the cement from aqueous ſolution or ſuſpenſion. Suppoſe theſe pebbles to have been placed in a ſituation ſimilar to that of petrified ſiliceous wood; its pores have been completely filled with ſiliceous matter, of the nature of agate; and, in like manner, the interſtices between theſe looſe pebbles may have been filled with this matter, and thus a conſolidated heterogeneous maſs might be formed.

Agates belong to the ſiliceous genus, and are conſidered by Dr. Hutton as affording, from their ſtructure, an argument in favour of his theory. They are maſſes, the internal parts of which have a certain arrangement, which, it is alleged, could not be produced by the infiltration of any fluid, but which muſt have ariſen from the circumſtances under which the agate was formed.

* Illuſtrations, &c. p. 26.

That the fluidity from which agates have been conſolidated, has been that of fuſion, it is ſaid, is evident, becauſe " the formation of the " concentric coats, of which the agate is uſual- " ly compoſed, has evidently proceeded from " the circumference toward the centre, the ex- " terior coats always impreſſing the interior, " but never the reverſe. The ſame thing alſo " follows from this other fact, that when there " is any vacuity within the agate, it is uſually " at the centre, and there too are found the re- " gular cryſtals, when any ſuch have been form- " ed. It therefore appears certain, that the pro- " greſs of conſolidation has been from the cir- " cumference inwards, and that the outward " coats of the agate were the firſt to acquire " ſolidity and hardneſs.

" Now, it muſt be conſidered that theſe " coats are highly conſolidated; that they are " of very pure ſiliceous matter, and are utterly " impervious to every ſubſtance which we know " of, except light and heat. It is plain, there- " fore, that whatever at any time, during the " progreſs of conſolidation, was contained with- " in the coats already formed, muſt have re- " mained there as long as the agate was entire, " without the leaſt poſſibility of eſcape. But " nothing is found within the coats of the a- " gate ſave its own ſubſtance; therefore no ex-

" traneous ſubſtance, that is to ſay no ſolvent,
" was ever included within them. The fluidity
" of the agate was therefore ſimple, and unaſ-
" ſiſted by any menſtruum.

" In this argument, nothing appears to me
" wanting, that is neceſſary to the perfection of
" a phyſical, I had almoſt ſaid of a mathemati-
" cal, demonſtration. It ſeems, indeed, to be
" impoſſible that the igneous origin of foſſils
" could be recorded in plainer language, than
" by the phenomenon which has juſt been de-
" ſcribed *."

Notwithſtanding the ſtrong language in which theſe aſſertions are expreſſed, it may be demonſtrated, that agates could not have been formed by fuſion, but muſt have originated from the operation of water. Their ſtructure is ſo peculiar that there may be ſome difficulty in pointing out clearly the preciſe mode of their formation, but the following explanation by Werner, and which has alſo been ſuggeſted by Dolomieu and Kirwan, will be found to accord better with the appearances of the foſſils of this family, then the Huttonian hypotheſis.

During the conſolidation of the ſtrata, it is ſuppoſed that extrications of air muſt have taken

* Illuſtrations, &c. p. 78, 79.

place, which in a ſoft maſs would form vacuities of a ſpheroidal form; after the conſolidation had been completed, theſe vacuities are ſuppoſed to have been filled with a fluid holding the matter of chalcedony, jaſper, and other ſubſtances of which agates conſiſt, in ſolution. Theſe would be depoſited ſucceſſively from the various affinities of the ſubſtances diſſolved to the ſolvent; and in this manner the ſucceſſive coats would be formed, the exterior coat determining in a great meaſure the figure of the interior depoſited upon it. Theſe coats, it has been obſerved, vary in the purity or homogeneous nature of their ſubſtance, the outer is compoſed of the coarſeſt or moſt heterogeneous ſubſtance, as of jaſper or carnelion; as it approaches to the centre, it generally becomes purer, till at length from the ſolution thus purified as it were, by theſe depoſitions, cryſtals of quartz and amethyſt ſhoot. When the ſolid matter had been completely ſeparated from the fluid, the latter would eſcape by percolation (for the hardeſt foſſils of this claſs, contrary to the aſſertion of Dr. Hutton, are capable of allowing water to paſs through them): if the orifice by which the ſolution entered was cloſed up by the depoſition, a hollow agate would be formed; if it were not, ſucceſſive portions of the ſolution

might find acceſs, and the whole cavity be completely filled.

From this hypotheſis, the general appearances of agates can be explained; and there is one variety, that of the hollow agate, which can ſcarcely be explained by any other, or which at leaſt can receive no explanation from the Huttonian hypotheſis. The cavities in theſe agates are ſuppoſed, according to the latter hypotheſis, to ariſe from the contraction of the maſs in its conſolidation and cooling. But there are many of them in which the cavity is ſo diſproportioned to the ſolid cruſt, that it could not poſſibly have ariſen from this cauſe. Thus they may be found in which the cavity is four inches in diameter, while the ſolid external coat is not more than $\frac{1}{2}$ inch thick: it is obvious that no ſhrinking of this maſs could have produced a cavity of this kind. The explanation again by the Wernerian hypotheſis, in which the production of the cavity is referred to the extrication of a portion of ærial fluid, and the ſubſequent depoſition of the matter of which the cruſt is formed, is perfectly ſatisfactory; at the ſame time it cannot be adapted to the Huttonian hypotheſis, for according to that ſyſtem, the fuſion of the ſtrata has taken place under an immenſe compreſſion, by which every extrication of air is prevented, and ſuch a preſſure

muſt, even by the defender of that hypotheſis, be ſuppoſed preſent in the formation of theſe hollow agates, becauſe they frequently contain carbonate of lime, which, but for this circumſtance, muſt have been decompoſed.

There is another appearance in certain agates which Dr. Hutton adduces as an argument in ſupport of his theory, and which affords a proof of its falſity. In agates of chalcedony, he obſerves, calcareous ſpar is often incloſed, and theſe are found mutually impreſſing and impreſſed by each other; "the angles and planes "of the ſpar are indented into the chalce-"dony, and the ſpherical ſegments of the chal-"cedony are imprinted on the planes of the "ſpar. Theſe appearances are conſiſtent with "no notion of conſolidation that does not in-"volve in it the ſimultaneous concretion of the "whole maſs; and ſuch concretion cannot a-"riſe from precipitation from a ſolvent, but "only from the congelation of a melted "body *."

Preciſely the oppoſite concluſion may be drawn, with much more juſtice,—that this ſimultaneous conſolidation could not take place from the fluidity produced by fuſion; for ſubſtances of different natures have always different degrees

* Illuſtrations, &c. p. 247.

of fuſibility, and, having this difference, muſt become ſolid at different temperatures. Either the chalcedony muſt have been more fuſible than the calcareous ſpar, or the ſpar than the chalcedony; and, which ever be the caſe, the one that was leaſt fuſible muſt have concreted firſt, and might take its peculiar form, but they never could concrete ſimultaneouſly ſo as to impreſs and be impreſſed by each other; and ſince Dr Hutton not only allows but contends for this mutual impreſſion, he eſtabliſhes a fact which overturns his own hypotheſis of the formation of theſe foſſils.

It is indeed ſingular that this caſe of ſimultaneous conſolidation or cryſtallization ſhould have ever been employed as a proof that it had taken place from fuſion, as in various other caſes beſide that of agates it is, for it implies a ſuppoſition which it is eaſy to demonſtrate is falſe. If two or more different ſubſtances be in fuſion, it is evident that they will become ſolid, according to their fuſibilities; the leaſt fuſible, requiring the higheſt temperature to preſerve it ſolid, will, on a reduction of temperature, firſt paſs to the ſolid ſtate, and will be ſucceeded by the others, which are more fuſible. It cannot be ſuppoſed that two ſubſtances ſhould, from a ſtate of fuſion, become ſolid preciſely at the ſame time, ſo as to impreſs each other, unleſs they

are of preciſely the ſame degree of fuſibility. Now there are not, perhaps, two ſubſtances in nature with reſpect to which this is the caſe. At leaſt, if it happened with any two, it might be conſidered, both as a ſingular coincidence, and ſtill more as the reſult of a very extraordinary combination of circumſtances, that of all bodies the two which thus happened to agree in fuſibility ſhould have been brought together in fuſion. But it may ſafely be affirmed, that not another example of it would be met with, far leſs that it ſhould take place with reſpect to many ſubſtances; and if it ſhould actually be found in nature that there were a variety of groupes or aggregates of foſſils which mutually impreſſed each other, this, inſtead of being regarded as a proof that theſe ſubſtances had conſolidated from ſimple fuſion, would furniſh the cleareſt demonſtration that the fluidity from which they had become ſolid muſt have been of a different kind.

It may be ſaid, perhaps, that it is not clear how from ſolution two different ſubſtances ſhould conſolidate or cryſtallize at the ſame time. Let it be granted, that it is not obvious how this ſhould happen; yet ſtill we have no demonſtration, as in the former caſe, that this could not be the caſe; and therefore, as it is the only ſuppoſition left to us, it ought to be admitted. But

we need not reſt ſatisfied with this argument. It is poſſible to conceive how different ſubſtances in ſolution in one fluid may be brought either to cryſtallize or conſolidate together, or to give the appearance of ſimultaneous conſolidation. Let it be imagined, that by ſome alteration of circumſtances in the ſolution the cryſtallization of one of the ſubſtances was occaſioned: it is conceivable that the ſeparation of this ſubſtance might ſo alter the exiſting attractions, by which one or other of the remaining ſubſtances in the ſolution were held diſſolved, that it alſo might inſtantly begin to ſeparate, and thus its conſolidation would be ſimultaneous, or very nearly ſo, with that of the other, and they might impreſs each other: Or, as cryſtallization is promoted by a *nucleus*, and ſtill more by the preſence of a body already cryſtallized, it is conceivable, that if in a ſaturated ſolution of different ſubſtances, one of them ſhould by an alteration of circumſtances, be brought to cryſtallize, the cryſtallization of the others might inſtantly commence, as we ſee take place when we drop a ſolid into a ſaturated ſolution of a ſalt; and thus the moſt intimate admixture and mutual penetration of cryſtals might take place. Nay, it is no improbable ſuppoſition, that by the ſudden admiſſion of air to ſuch a ſaturated fluid, or even by the ſudden evolution of any

gas from it, the inſtantaneous cryſtallization of more than one of the ſubſtances diſſolved in it might be produced.

It is thus evident, that various ſuppoſitions ſufficiently probable can be made, by which the ſimultaneous conſolidation of different ſubſtances from a ſtate of ſolution may be accounted for. It cannot, therefore, be ſaid, that this is a difficulty which cannot be explained, and which preſſes againſt the theory. On this point the Neptunian has every advantage over the Huttonian hypotheſis. It can be demonſtrated, that according to the principles of the latter no ſuch thing as ſimultaneous conſolidation could take place. No demonſtration of this kind can be brought againſt the other, which at once, therefore, gives it the ſuperiority. But it can claim a ſtill higher advantage, ſince it is even eaſy to ſhew, in perfect conformity with its principles, how this ſingular operation of ſimultaneous conſolidation might take place. Every inſtance of it, therefore, ſuch as that of chalcedony and calcareous ſpar in agates, and others to be afterwards noticed, are ſo many proofs of the formation of foſſils by ſolution.

" The common grit, or ſandſtone, though it " certainly gives no indication of having poſ- " ſeſſed fluidity, is ſtrongly expreſſive of the ef- " fects of heat. It is ſo, eſpecially in thoſe in-

" ſtances where the particles of quartzy ſand, " of which it is compoſed, are firmly and cloſe- " ly united, without the help of any cementing " ſubſtance whatſoever *." The Neptunian would ſay, that this is mere aſſertion and no argument whatever, as the conſolidation of this ſtone, the denſity or compactneſs of which is not great, might be produced by the mutual attraction exerted between its finer particles; or, as all ſandſtone contains a portion of argil or lime, theſe might alſo ſerve, in part at leaſt, as the connecting medium.

If we ſhould, *a priori*, expect, from an examination of the ſtrata of the earth, any indication of their origin, or of the nature of the agents to whoſe operation they had been ſubjected, it is certain that none leſs ambiguous could be obtained than that afforded by the animal and vegetable impreſſions and remains which they contain. Theſe at once afford the double proof, that they have been depoſited from water, and that they have not been ſubjected to the action of fire.

This proof we ſhall find in all the ſtrata, more ſparingly perhaps in thoſe of the Siliceous Genus than in the others, but ſtill in thoſe ſo abundantly as to furniſh the moſt concluſive

* Illuſtrations, &c. p. 259.

evidence. Beſides the petrifactions in flint, which have been already noticed, they occur in petroſilex, and ſiliceous ſandſtone. It is obvious, that the intenſe heat which, according to the Huttonian theory, is applied to theſe foſſils for their conſolidation, ought to have ſoftened or fuſed the ſubſtance of theſe petrifactions. Granite and whin, in that theory, are ſuppoſed to be formed nearly from the ſame materials as theſe ſtrata,—from ſubſtances depoſited at the bottom of the ſea; and the reaſon given why they contain no petrifactions, or no remains of marine animals, is, that by the heat by which the whin or granite has been fuſed, they have been deſtroyed. But the heat neceſſary even to ſoften petroſilex, or ſiliceous ſandſtone, is much greater than that requiſite to fuſe whin; and therefore, *a fortiori*, theſe foſſils ought not, more than whin, to contain organic remains. This follows ſtill more ſtrongly from the fact that the ſiliceous and calcareous earths act mutually as fluxes, ſo that if a ſhell were involved in ſandſtone, ſoftened by heat, this alone ſhould cauſe them to combine.

Among the foſſils belonging to the CALCAREOUS GENUS, are ſtated, as proving the action of ſubterranean heat, " the calcareous breccias, " compoſed of fragments of marble or lime- " ſtone, and not only adapted to each other's

" ſhape, but indented into one another, in a " manner not a little reſembling the ſutures of " the human cranium. From ſuch inſtances, " it is impoſſible not to infer the ſoftneſs of " the calcareous fragments when they were con- " ſolidated into one maſs. Now, this ſoftneſs " could be induced only by heat; for it muſt be " acknowledged that the action of any other " ſolvent is quite inadequate to the ſoftening " of large fragments of ſtone, without diſſolving " them altogether *." This appearance of theſe marble breccias can ſcarcely be accounted for on the ſuppoſition of their being ſoftened by heat; for in the joinings of the fragments the ſharp angles of many of them are often preſerved at their inſertions. In general, theſe fragments are connected and interlaced, as it were, by a common ground or cement, and not indented into the ſubſtance of each other, and this connecting cement may have been depoſited from water around theſe fragments. Where there is an appearance of indentation in ſome places, it might ariſe from the accidental poſitions of the fragments, and the preſſure from the gravity of the maſs, by which the ſharp projections have been inſerted into hollows or fractures of the others, and united by

* Illuſtrations, &c. p. 28.

the common cement. Nay, the juſtneſs of the aſſertion in the objection may be denied; for it is poſſible, that by the continued application of a ſolvent to theſe fragments, they might partly be diſſolved, partly ſoftened. They might thus be indented together by preſſure, and a cement at the ſame time formed by which they would be conſolidated into one maſs.

"In many other inſtances it appears certain, "that the ſtones of the calcareous genus have "been reduced by heat into a ſtate of fluidity "much more perfect. Thus, the ſaline or finer "kinds of marble, and many others that have a "ſtructure highly cryſtallized, muſt have been "ſoftened to a degree little ſhort of fuſion, be-"fore this cryſtallization could take place. Even "the petrifactions which abound ſo much in "limeſtone tend to eſtabliſh the ſame fact; for "they poſſeſs a ſparry ſtructure, and muſt have "acquired that ſtructure in their tranſition from "a fluid to a ſolid ſtate *."

In the firſt of theſe inſtances, there is merely the gratuitous aſſumption that the cryſtallization producing the ſparry appearances of theſe marbles, muſt have been from fluidity induced by fuſion, and not from fluidity occaſioned by ſolution. The latter affords an example of a very

* Illuſtrations, &c. p. 29.

ſingular mode of reaſoning. It is acknowledged, that the petrifactions in limeſtone muſt have been fuſed, becauſe they could only acquire their ſparry ſtructure in their tranſition from a *fluid to a ſolid ſtate*, and of courſe the figure and the ſtructure of the ſubſtances petrified, which are generally ſhells, muſt have been loſt. But how could this figure be reſumed when the fuſed ſubſtance returned to the ſolid ſtate. If a ſhell in a maſs of limeſtone had been in fuſion, it is obviouſly impoſſible that in becoming ſolid it could again aſſume its preciſe arrangement and figure.

The proof from the preſence of theſe petrifactions in calcareous ſtrata, in favour of the Neptunian theory, is concluſive. Theſe ſtrata are ſuppoſed to be formed in the bed of the ſea, and probably chiefly from the decay of marine animals. Their ſhells conſiſt principally of carbonate of lime; the animal matter which is mixed with this is evolved by putrefaction during their ſlow conſolidation, and its place is ſupplied by a portion of the carbonate held in ſolution by the ſurrounding water: from a ſimilar depoſition, mixed with ſmaller portions of argillaceous earth, oxyd of iron and other ſubſtances, their conſolidation is completed, and the more ſlow depoſition of pure carbonate of lime, gives riſe to the formation of the ſpar and cryſtals which theſe ſtrata always contain.

In the Huttonian theory, the preſervation of theſe animal remains is a problem which is not ſolved. It is maintained, that conſolidation cannot be effected but by fuſion, partial or complete; and of courſe it is maintained, that theſe ſtrata of limeſtone and marble have been melted, or at leaſt ſoftened by heat. It remains, then, to be explained, how the ſhells of the marine animals, which theſe ſtrata contain, have eſcaped the action of this heat; how their figure and texture have not been deſtroyed? Their compoſition is the ſame as that of the remaining matter of the limeſtone or marble; and indeed theſe ſtrata are ſuppoſed, by Dr. Hutton, to originate from the remains of ſea animals. If theſe, therefore, were in a fuſed or ſoftened ſtate, ought not the included ſhells to have ſuffered preciſely the ſame change? The fuſion of theſe ſtrata muſt, according to the Huttonian, have been nearly complete, for they have the ſparry ſtructure, and contain large veins and ſpots of perfect ſpar. It is impoſſible to conceive, how ſuch a degree of fuſion ſhould have taken place and theſe ſhells not have been at leaſt ſo far ſoftened, as to have loſt ſomewhat of their figure, and of the ſharpneſs and diſtinctneſs of their angles and lines. Nay, the cavity of the ſhell is often filled with regular cryſtals of carbonate of lime, which, if formed by heat, muſt

have concreted from a ſtate of perfect fuſion: but this fuſed matter could not have remained in contact with the ſhell, without melting it completely or partially; and, therefore, ſuch appearances, which, ſo far from being rare, are extremely common, are incompatible with ſuch a ſuppoſition.

The concluſion becomes ſtill more evident, when we conſider the extreme delicacy often obſerved in theſe impreſſions. The calcareous ſtone of Monte Bolca is known to every mineralogiſt, for the large and perfect impreſſions of different kinds of fiſh found in it. Theſe are numerous, the entire figure of the fiſh is impreſſed upon it, and with ſuch accuracy, that the characters of a number of ſpecies have been eſtabliſhed. It is abſolutely incredible that an impreſſion of this kind could remain on a ſtone ſubjected to an intenſe heat, that the matter of the fiſh ſhould not have ſuffered the ſmalleſt decompoſition, or even mechanical derangement; and the bare inſpection of theſe ſpecimens will be ſufficient to convince the unprejudiced obſerver, that theſe ſtrata could not have owed their conſolidation to fire: but there is no difficulty in conceiving, that water, by that ſlow agency above explained, might form ſuch petrifactions.

Another fact reſpecting theſe foſſils, which cannot be explained in the Huttonian hypotheſis, has been ſtated by Mr. Kirwan,—the abſence of phoſphoric acid in the finer kinds of marble and limeſtone. The ſhells of marine animals contain a portion of phoſphat of lime in their compoſition, as has been eſtabliſhed by the experiments of Mr. Hatchet; this ſubſtance, Mr. Kirwan juſtly remarks, is indeſtructible by heat; and, therefore, had theſe marbles been formed from the remains of marine animals, they ought to contain the full proportion of phoſphat of lime, which theſe contain. The reply which Mr. Playfair gives to this, is extremely obſcure. To give the argument force, he obſerves, it would be neceſſary to prove, that phoſphoric acid exiſts in theſe limeſtones which evidently conſiſt of ſhells in a mineralized ſtate: if theſe are found without phoſphoric acid, it is evident that the preceeding argument fails entirely. But this proof is not neceſſary. The marble in which no phoſphoric acid has been found is ſuppoſed, by the Huttonian geologiſt, to be formed from remains of marine animals equally with any other; and the circumſtance of other marbles or limeſtone, containing or not containing phoſphat of lime, is of no conſequence. If it be found, that other limeſtones

do not contain phoſphat of lime, the argument will indeed become more general; but, however this may be, the objection, from the particular fact with regard to the marbles which have been analyſed, remains the ſame. To the Neptunian theory, the objection does not apply, becauſe the ſmall quantity of phoſphat of lime preſent may be carried off by water.

It is, laſtly, to be remarked, with regard to the calcareous foſſils, that the evidence for the formation, of at leaſt ſome of them by water, is ſo unexceptionable, that it muſt be admitted even by the Huttonian. Thus, calcareous cryſtals are often found in ſituations where they muſt neceſſarily have cryſtallized from a ſolution of carbonate of lime in water. They are thus met with lining the cavities of ſhells, ſuch, for example, as the ſhell of the Cornua Ammonis, and many others. Theſe ſhells themſelves conſiſt of carbonate of lime; and of courſe they could not contain carbonate of lime in perfect fuſion, without ſuffering an alteration. The ſtate of fluidity, therefore, from which theſe cryſtals have concreted, has not been that from fuſion, but muſt have been that of ſolution in water. Another example, not leſs equivocal, is that of calcareous ſtalactites. Theſe have not only in general the ſparry texture, but are often covered with cryſtals, and their for-

mation in the humid way is abundantly obvious. The force of this concluſion is evident. It is proved, that calcareous cryſtals, different in no reſpect from others found in nature, are, in ſome caſes at leaſt, formed from ſolution; that they are ever formed by the fuſion of carbonate of lime, is a mere hypotheſis, ſupported by no direct evidence;—till ſuch evidence, therefore, be adduced, the Neptunian has a right to regard that which he brings forward as concluſive, and to conſider all calcareous cryſtals as being formed in the ſame mode; and to this concluſion nothing but a ſimple poſſibility can be oppoſed. Even that conjecture would be, *a priori*, improbable, ſince it would be ſingular, if this foſſil ſhould be capable of being formed both by fire and water, and ſhould receive from each preciſely the ſame form of cryſtallization. And, if it be rendered probable, that calcareous cryſtals are formed by ſolution, the ſame mode of formation muſt be aſſigned to almoſt every foſſil; for there are very few with which theſe cryſtals are not ſo intimately aſſociated, that the ſame origin muſt neceſſarily be given to them all.

Among the foſſils of the ARGILLACEOUS GENUS, the variety of argillaceous iron ore, named the ſeptaria affords an argument on which Dr Hutton laid much ſtreſs. This foſſil is generally

found in ſpheroidal nodules, which when broken exhibit a ſingular ſtructure. The ground or baſe conſiſts of argillaceous iron ore, but this is divided into a number of ſepta by veins of calcareous ſpar, which ſhoot from the centre of the ſtone to the circumference, but do not reach it. This proves, it is ſaid, that the calcareous matter has not been introduced into the ſtone from without by infiltration. "The only "other ſuppoſition that is left for explaining the "ſingular ſtructure of this foſſil is, that the "whole maſs was originally fluid, and that in "cooling the calcareous part ſeparated from the "reſt, and afterwards cryſtallized *."

It might be granted, that the ſtructure of this ſtone proves that it muſt have been in a fluid ſtate, and that in hardening, the contraction of the baſe, and the ſeparation of the calcareous matter, had taken place at the ſame time, without its following as a neceſſary conſequence that this fluidity muſt have ariſen from fuſion. It may have been a depoſite from water, and the ſhrinking and ſeparation may have taken place during its conſolidation; or it might be ſuppoſed, with perhaps equal probability, that the argillaceous iron ore only had been depoſited; that in conſolidating it had ſhrunk and ſplit internal-

* Illuſtrations, &c. p. 26.

ly; and as this is actually a porous ſtone, if it were afterwards expoſed to water having carbonate of lime in ſolution, the water might enter by infiltration through its ſubſtance, and the cavities be filled, as far as they extended, with calcareous matter. That theſe ſeptaria have had an aqueous and not an igneous origin, is proved by their ſometimes containing impreſſions of organic ſubſtances, particularly ſhells.

In the argillaceous ſtrata, petrifactions are extremely frequent, more particularly ſhells and impreſſions of vegetables. Theſe are found in clay, argillaceous ſhiſtus, argillaceous ſandſtone, argillaceous ironſtone, and various others. The argument with reſpect to them, is the ſame as with regard to the other ſtrata in which they are found. The operation of fire ought to have altered or deſtroyed theſe remains and impreſſions, nor is any cauſe pointed out in the Huttonian theory by which they could be preſerved.

This argument is concluſive, whether we conſider the deficiency of explanation in the one theory, or the ſatisfactory ſolution afforded by the other. Let the example of marine ſhells preſerved in ſandſtone be taken. If that ſandſtone has been formed by depoſition from water, it might happen that the ſhells of animals exiſting in that water, might be involved in the de-

poſite; and, in this caſe, no change would happen to them, except perhaps their being more or leſs impregnated with particles of the matter in which they were depoſited. This is accordingly the ſtate in which theſe ſhells are found, and the actual appearance exactly correſponds with that which the theory would lead us to expect. If, again, the ſtratum of ſandſtone were conſolidated by heat, it is impoſſible to conceive how that heat ſhould operate without changing the figure or ſtructure of the contained ſhells. Nay, in this example, another circumſtance is preſent, which ſtill more forcibly proves, that heat could not thus be applied without producing ſome change. Argillaceous ſandſtone conſiſts principally of ſiliceous and argillaceous earth: now theſe ſerve as a flux to calcareous earth, and cauſe its fuſion at a temperature much lower than that requiſite to fuſe it when pure. Shells of marine animals have this earth for their baſis; and had heat been applied to them, ſurrounded by argillaceous ſandſtone, ſuch a combination muſt have been effected.

Or, let the argument be conſidered with reſpect to the vegetable impreſſions, which are often found on ſhale or ſhiſtus. Theſe are ſo delicate and perfect, that the genus, and even the ſpecies of the vegetable, can often be determined, from the preſervation of the moſt deli-

cate foliage and flower. It may be aſked of the Huttonian, if theſe ſtrata were conſolidated by fire, as he ſuppoſes, how the vegetable matter could be entirely removed, while the impreſſion ſhould remain perfect and unimpaired? The vegetable matter ought undoubtedly to have ſuffered decompoſition; for, even if the eſcape of its volatile parts were prevented by the preſſure preſent, ſtill its principles muſt, under ſuch a heat, have entered into new combinations, by which their ſtructure muſt have been altered, or, at all events, by the fuſion or ſoftneſs of the ſtone on which the vegetable matter lay, its figure muſt have been changed.

Among the BITUMINOUS SUBSTANCES, Coal is brought forward as affording a proof of the igneous origin of foſſils. Inſtead of doing ſo, it will be found, like the preceding examples, to afford the ſtrongeſt objections to the Huttonian hypotheſis.

From the nature of this ſubſtance, and the appearances of its ſtrata, there can be little doubt that it is principally of vegetable origin. The moſt probable theory of its formation is, that vegetable matter carried to the ſea, has, by the direction of currents, been depoſited in banks, and that during this ſubmerſion it has ſuffered that ſlow kind of decompoſition by which the greater part of its principles have

been evolved in new combinations, while its carbon, with a portion of hydrogen, have remained; and this, mixed with more or leſs earthy matter depoſited at the ſame time from the ocean, has in its ſoft ſtate been conſolidated by the force of aggregation, and has formed coal. The decompoſition by which this has been effected, is probably analogous to that which we know animal matter, when immerſed in water, ſuffers. Its hydrogen, azot, and oxygen, enter into various combinations, forming gaſes which eſcape; and its carbon retaining, by a chemical attraction, a portion of hydrogen in combination, remains, forming a ſpecies of fat. Carbon is ſtill more abundant in vegetable than it is in animal matter; and this conſtitutes the principal difference between them. Vegetable matter, however, is liable to ſimilar decompoſitions; and under the circumſtances pointed out, it is reaſonable to believe that changes of a ſimilar kind, modified as to the reſult by the difference in the proportions of its principles, will take place: in other words, its reſidue will be more carbonaceous, but ſtill with a proportion of hydrogen, ſo as to render it more or leſs bituminous. We accordingly find, that wood, by immerſion in water, becomes firſt brown, and then black; and the ligneous fibre, by ſlow decompoſition, is com

pletely converted into a black mould, in which carbon predominates. It is eaſily conceivable, that this proceſs being carried on under different circumſtances, may proceed with various degrees of rapidity, and to a greater or leſs extent. Hence will originate different varieties of coal, ſome being much more carbonaceous than others, while their compoſition is alſo varied by the different quantities of earth depoſited during their formation.

It is not improbable that ſome ſpecies of coal may likewiſe have a different origin. It may be ſuppoſed, and indeed muſt be, that carbon, in common with other ſimple ſubſtances, exiſted in the chaotic fluid; and this carbon, combined with a portion of oxygen, may have formed a variety of coal which was precipitated. This probably forms the coal, which is infuſible and very little inflammable, the mineral carbon, or anthracite, and which is conſidered by many mineralogiſts as very remote from a vegetable origin. From its analyſis it does not appear to contain any bitumen, its principles, according to the experiments of Dolomieu and Panzenberg, being pure carbon, or at leaſt oxyd of carbon, ſilex, argil, and oxyd of iron *: and it is obſerved by Dolomieu, that this foſſil, as well as graphite or plumbago, is found in the primitive mountains,

* Brochant, t. 2. p. 81.

generally in veins. The ſuppoſition offered reſpecting the origin of this variety of coal, is alſo confirmed by the fact, that carbon is found more or leſs oxydated in the compoſition of ſeveral primitive foſſils and rocks.

Dr. Hutton, however, conſiders coal as a ſubſtance formed by the operation of ſubterraneous fire; and ſuppoſes that there are ſeveral appearances connected with it, which prove it to have had ſuch an origin. It is ſomewhat difficult to give a clear ſtatement of his theory on this ſubject; but it ſeems to be comprehended under the following propoſitions:

Firſt, It is conceived, that an intenſe heat has been applied to vegetable matter at the bottom of the ocean, and that at the ſame time no great degree of preſſure has been preſent. By this operation the vegetable matter has been charred, or converted into one ſpecies of coal, while the more volatile inflammable matter of the wood has been diſengaged. *2dly*, This volatiliſed matter being of a bituminous nature, is ſuppoſed to be diffuſed in the ocean, and to be "employed in forming other ſtrata, which were "then depoſited at the bottom of the water." To this would be added, "all the fuliginous "matter that is formed in burning bodies upon "the ſurface of this earth, which is firſt deliver-"ed into the atmoſphere, but ultimately muſt

" be ſettled at the bottom of the ſea *." And, *laſtly*, Another ſupply of bituminous matter is derived from the vegetable ſubſtances diſſolved or ſuſpended in the water of the rivers, and brought to the ocean. The bituminous matter from theſe ſources is ſuppoſed to be precipitated either alone, or with a portion of fine earth, likewiſe ſuſpended in the water of the ſea; and this precipitate, being afterwards conſolidated by ſubterraneous heat, forms ſtrata of pure foſſil coal.

It would be an irkſome, and a very unneceſſary taſk, to enter on the formal refutation of theſe various ſuppoſitions. A few obſervations on the moſt palpable deficiencies of the theory will be ſufficient.

It may, in the firſt place, be obſerved, that there are no direct arguments adduced to eſtabliſh the igneous origin of coal. The theory is attempted to be eſtabliſhed principally by arguments drawn from its connection with other foſſils, which it is to be proved have had an igneous origin. Thus, becauſe " the beds of coal " are diſpoſed in the ſame manner, and are al- " ternated indiſcriminately with thoſe of all the " ſecondary rocks," it is inferred that they muſt have been formed by the ſame operation,

* Theory of the Earth, vol. I. p. 577.

and that this has been fuſion by heat, a concluſion founded on nothing peculiar to coal but on the evidence reſpecting the origin of theſe ſecondary rocks. They are alſo ſaid to be "traverſed like the other ſtrata by veins of all the "metals, of ſpar, of baſaltes, and of other ſubſtances *." But here likewiſe the argument is not direct, but depends entirely on its being proved that theſe veins have been formed by fuſion. Laſtly, the coal ſtrata are ſaid to "contain "pyrites in great abundance, a ſubſtance that "is, perhaps, more than any other the decided "progeny of fire *." But this, equally with the preceding arguments, reſts on a proof with reſpect to the origin of a different ſubſtance,—a ſubſtance which it will immediately be ſhewn there is every reaſon to conclude has been formed in the humid way.

From this ſtatement, it is obvious that no direct argument from the properties or appearances of coal, is adduced in proof of its igneous origin.

Not only, however, is it unſupported; ſeveral of the ſuppoſitions it involves are highly improbable. Thus, it is imagined that a great part of the coal ſtrata are derived from bitumen produced either by the expoſure of wood to ſubterraneous heat, by the burning of vegetable mat-

* Illuſtrations, &c. p. 33.

ter at the ſurface, or by the ſolution or ſuſpenſion of the oily and inflammable parts of vegetables in the water of rivers; this bitumen from theſe ſources being ſuppoſed to be diffuſed in the ocean, and precipitated ſo as to form ſtrata. But how is this matter to be collected in one place, and, if collected, how is its precipitation to be effected. It is lighter than water, eſpecially ſea water: it muſt therefore remain at the ſurface, and no cauſe can be aſſigned for its being carried to the bottom.

The peculiar modifications of preſſure ſuppoſed requiſite for the igneous formation of coal, furniſh another argument againſt the theory. "It muſt be conſidered," ſays Dr. Hutton, "that "while immerſed in water, and under inſuper-"able compreſſion, the vegetable, oily, and reſin-"ous ſubſtances, would appear to be unalterable "by heat; and it is only in proportion as cer-"tain chemical ſeparations take place that theſe "inflammable bodies are changed in their ſub-"ſtance by the application of heat. Now, the "moſt general change of this kind is in conſe-"quence of evaporation, or the diſtillation of "their more volatile parts, by which oily ſub-"ſtances become bituminous, and bituminous "ſubſtances become coally *." This, then, is

* Theory of the Earth, vol. i. p. 70.

the ſuppoſition which Dr. Hutton chooſes to make, and which, indeed, appears to be a neceſſary one for his theory, that coal is formed by heat applied to vegetable matter under different degrees of preſſure, the difference in this reſpect producing a difference in the reſult, but that in all caſes the preſſure muſt be ſuch as to allow of ſome ſeparation of volatile principles.

It might be granted, that this abſence or diminution of preſſure might occur in certain ſituations; but how does it happen that this indiſpenſable condition ſhould invariably be found in the ſubterranean regions, when heat is to be applied to vegetable matter. If the uſual preſſure were preſent, that matter, according to Dr. Hutton, would remain unchanged with reſpect to compoſition by any heat; but, in nature, we meet with no collections or ſtrata of vegetable matter in this peculiar ſtate, in which they have ſuffered heat without being changed, yet, ſurely, ſince heat is applied to all the other ſtrata under immenſe preſſure, it ought alſo to have been occaſionally applied under a ſimilar preſſure to the matter of coal. The key to this apparent myſtery is, however, eaſily found. The common ſtrata are ſuppoſed to have been heated under an immenſe preſſure, which is not in any caſe ſuppoſed to have been abſent, *becauſe* the preſence of that preſſure is neceſſary, in many

caſes, to obviate certain objections to the ſuppoſition of their igneous origin, and, in all, an uniformity is, if poſſible, to be preſerved. The matter of coal, again, is ſuppoſed to have had heat applied to it *always* under a diminiſhed preſſure, *becauſe* that circumſtance is neceſſary, in the Huttonian theory, to account for its formation. The bare ſtatement of theſe accommodating coincidences is ſufficient to prove that they are merely fictitious, and that theſe are ſuppoſitions arbitrarily made, becauſe they are neceſſary in the theory.

Another difficulty may be ſtated againſt this hypotheſis of the formation of coal. Pyrites, or ſulphuret of iron, is ſuppoſed to be formed by heat, and this heat is ſuppoſed to have been applied under ſuch a preſſure, as has prevented the ſulphur, which is a very volatile ſubſtance, from having been driven off from the iron. Since pyrites occur, therefore, ſo abundantly in coal, a dilemma is preſented to the Huttonian geologiſt. If he ſuppoſe the vegetable matter of coal to have been fuſed under an immenſe preſſure, capable of reſiſting the eſcape of its volatile parts, he contradicts the author of the theory, who tells us, that under ſuch a preſſure, that matter could not have been formed into coal, a propoſition which, according to his hypotheſis, appears to be perfectly juſt. If he ſup-

poſe the heat to have been applied under a diminiſhed preſſure, ſo as to allow of the decompoſition of the vegetable matter, and the expulſion of its volatile principles, he will have the taſk of explaining how, in ſuch a ſituation, pyrites could be formed and cryſtallized. Nay, the difficulty is ſtill greater, for this ſubſtance is found in that ſpecies of coal which is little inflammable, and which, according to the Huttonian theory, is ſuppoſed to have had heat applied to it under ſo little preſſure, that all its bituminous matter had been expelled. Mr. Kirwan relates, that the Kilkenny coal, which is of all others the moſt completely deſtitute of bituminous matter, contains pyrites*; and Dr. Hutton himſelf mentions a ſpecimen being in his poſſeſſion of plumbago, (which he conſiders as the laſt of the ſeries, or as coal completely deprived of bitumen) ſtudded with pyrites †. The explanation of theſe appearances, according to the Huttonian ſyſtem, involves a direct contradiction in terms. To account for the formation of this ſpecies of coal, it is ſaid to have been fuſed with an entire abſence of preſſure, ſo that all its bituminous matter has eſcaped. Pyrites, again, is a ſubſtance ſaid to be formed by fuſion, but under a ſtrong preſſure, by which its ſulphur, a

* Geological Eſſays, p. 473. † Theory of the Earth, vol. i. p. 616.

ſubſtance at leaſt as volatile as bitumen, is kept in combination with the iron. It would, therefore, according to this theory, be impoſſible that coal of this kind and pyrites ſhould exiſt together, the circumſtance ſuppoſed neceſſary for the formation of the one, being that which muſt inevitably have deſtroyed the other.

A ſimilar argument, equally forcible, may be deduced from the connection of limeſtone with coal. No arrangement is more common in geology than that of limeſtone alternating with coal, or covering it. The formation of limeſtone, in the Huttonian ſyſtem, is aſcribed to fuſion, under vaſt compreſſion, by which the carbonic acid has been retained in combination with the lime; and the advantage which the admiſſion of this modifying circumſtance gives to the Huttonian geologiſt in his reaſonings, is very amply, perhaps oſtentatiouſly diſplayed in the defence of the ſyſtem. It is always admitted, however, that but for this circumſtance of compreſſion, the conſolidation of limeſtone by fuſion could not be accounted for. The formation of coal, on the other hand, it is contended, cannot take place but when preſſure is diminiſhed or withdrawn. When limeſtone covers coal, it can have been conſolidated only by the heat operating through the coal. But if preſſure were preſent on the limeſtone above,

how could it have been abſent from the coal beneath? To ſuppoſe this is a palpable abſurdity; and therefore, were the Huttonian theory true, either the coal ſhould not have been formed, or the limeſtone ſhould have been converted into lime.

Among the SALINE SUBSTANCES, Rock Salt is adduced as affording an argument in favour of the Huttonian theory. This ſubſtance is found generally in ſtrata, and it is perfectly compact and indurated. This ſtate, it is ſaid, could not be produced by cryſtallization from water; a mere aſſemblage of looſe cryſtals, without ſolidity or coheſion, only could be formed; and to convert theſe into a firm and ſolid rock, would require the application of ſuch heat as was able to reduce it into fuſion. "The conſolidation of rock ſalt, therefore, cannot be explained but on the hypotheſis of ſubterraneous heat *."

The Neptuniſt will find no great difficulty in obviating this argument. If the cryſtallization has, from alteration in the circumſtances of the ſolution, been haſty, it is conceivable that, inſtead of an aſſemblage of ſmall regular cryſtals, large and compact cryſtalline maſſes might be formed; or even if the ſalt firſt depoſited had

* Illuſtrations, &c.

not been perfectly compact, its conſolidation might be completed by the percolation of water holding ſalt in ſolution through it, and depoſiting that ſalt in its pores.

It ſeems to be altogether inconceivable, how the formation of the immenſe ſtrata of ſea ſalt found in nature, can be accounted for on the Huttonian hypotheſis: All its uſual principles muſt in this caſe be relinquiſhed as utterly untenable. It cannot be ſuppoſed that this ſalt is derived, like the materials of all the other ſtrata, from the diſintegration of an ancient world; for allowing that ſtrata of ſalt exiſted in that world, and that theſe had ſhared in the general diſintegration, the ſalt muſt neceſſarily have been diſſolved by the water of the ocean, to which, in common with the other materials, it was carried: And how, therefore, could it be depoſited, ſo as to be ſubjected to the action of ſubterraneous heat? This is in fact acknowledged to be a caſe which cannot be explained in conformity to the general theory, and therefore a different hypotheſis is propoſed. "If the ope-"ration of ſubterraneous heat be admitted," ſays Profeſſor Playfair, "it appears poſſible, that the "local application of ſuch heat may have driven "the water in vapour from one place to another; "and by ſuch action often repeated in the ſame "ſpot, may have produced theſe great accumula-

" tions of ſaline matter that are actually found in " the bowels of the earth *."

This hypotheſis reſts on a ſeries of gratuitous aſſumptions, ſo extravagant, that, though one were admitted, the combination of the whole, and their adaptation to each other, may be regarded as impoſſible. By what cauſe, it may be aſked, was the central heat directed in its operation to this particular ſpot? Is it conceivable that any heat which it is poſſible could have thus been locally applied, would be ſufficient to convert the whole water of the ocean incumbent upon it into vapour, ſo as to occaſion the precipitation of its ſaline matter? By what cauſe had this heat, after producing ſuch an effect, ceaſed to operate, ſo as to allow the ſea again to cover the place from which it had been driven? And how ſhould this heat have again been brought to act preciſely on the ſame ſpot with the ſame force, ſo as to convert the ſea into vapour, and occaſion a new conſolidation of its ſalt, and that repeated even for a number of times? Any of theſe ſuppoſitions is ſo romantic as to be ſufficient to invalidate any hypotheſis in which it is received; but the combination ſo far exceeds the bounds of probability, that perhaps this might be ſelected as not inferior in

* Illuſtrations, &c. p. 37.

extravagance to any of thoſe ſuppoſitions which have been made in a ſcience celebrated as geology has been, for the wanderings of its cultivators beyond the regions of ſober reaſon.

It is, perhaps, unneceſſary to add thoſe facts which ſerve to refute ſuch an hypotheſis. It may be obſerved, however, that were it true, rock ſalt ſhould not contain the water of cryſtallization it does; it owes its very formation to a heat capable of driving off in vapour the immenſe quantity of water in which it was diſſolved, and of courſe this heat ſhould have driven off the water it might be diſpoſed to retain. If it had been formed in this manner, the remains of marine animals ſhould be abundant in it, which they are not; and laſtly, the ſaline matter depoſited ſhould have been preciſely that which ſea water holds diſſolved, muriate of ſoda with muriate of magneſia, and ſulphate of lime. But we find rock ſalt ſo little contaminated with theſe other ſalts, as to be purer even than the ſea ſalt obtained by artificial evaporation. This laſt fact at once demonſtrates the falſity of the hypotheſis; for it is too plain to require any illuſtration, that if the ſea in any part had been converted into vapour, the matter depoſited muſt have been a mixture of all the ſalts it held diſſolved.

It may be difficult, even on the Neptunian theory, to give a ſatisfactory explanation of the

origin of rock ſalt; but there are no appearances in this foſſil inconſiſtent with the ſuppoſition of its aqueous origin. The Neptuniſts have ſuppoſed, that it may have been formed by collections of the original ſea water in hollows, among the ſtrata, having ſuffered evaporation during the temporary retreat of the ocean, and that theſe having been ſucceſſively filled, have furniſhed the ſtrata of rock ſalt. But it is a ſtrong objection to this, and all other theories which derive it from the evaporation of ſea water, that it does not contain the ſaline ſubſtances which are diſſolved in that water, or at leaſt does not contain them in the due proportion, and that it contains fewer remains of marine animals than we ſhould expect, had it had ſuch an origin. Perhaps we may ſuppoſe, that the ſaline ſubſtances, in common with others, had exiſted in the original fluid in which all the materials of the ſtrata were diſſolved,—that part of theſe being locally accumulated, in the ſame manner as the materials of every other ſtratum have more or leſs been, from circumſtances which we cannot determine, had been confuſedly cryſtallized; and that any remaining portion had been retained in ſolution by the water, theſe ſalts of courſe remaining in it, in encreaſed proportion which were leaſt diſpoſed to cryſtallize. Such is the muriate of magneſia; and it is not impoſſible

but that even much of the muriate of ſoda now found in ſea water, may have been redifſolved from ſtrata formed. Or we may modify this explanation, by the equally probable ſuppoſition, that at firſt the ſoda and the muriatic acid of the ſalt had not been in combination, but that, in the courſe of the various alterations of attractions from the precipitations of the ſtrata, they had been brought together, had united, and if locally accumulated, cryſtallized. Such a ſuppoſition receives confirmation from the fact, that in many of the ſtrata, in trap for example, according to the excellent experiments of Dr. Kennedy, both ſoda and muriatic acid exiſt; and ſea ſalt itſelf is found ſometimes among primary ſtrata. This general hypotheſis, modified in either of theſe ways, involves no improbable ſuppoſitions, and is perhaps adequate to the explanation of the production of this foſſil.

Another ſaline ſubſtance, a variety of carbonate of ſoda found in Africa, is ſtated by Dr. Hutton as affording a proof of conſolidation from fuſion. This ſubſtance is ſuppoſed to have been part of the contents of a vein, as it has a ſtony cruſt adhering to one ſide of it; it has a ſparry ſtructure, and contains only about one-ſixth of the quantity of water of cryſtallization contained in the uſual cryſtallized ſtate of this ſalt. It is this laſt circumſtance which is con-

ſidered as affording a proof that this ſalt has not originated from water.

We are not told in what manner this ſalt might be ſuppoſed to be formed according to the Huttonian ſyſtem, and it ſeems impoſſible to give, according to the principles of that ſyſtem, any explanation of its origin. Every ſubſtance compoſing any ſolid part of the ſurface of the globe, is ſuppoſed to be derived from the waſte of a former world, and to have been depoſited from the ſea. But ſuppoſe carbonate of ſoda to have exiſted in the ſtrata of the former world, when diſſolved in the waters, and brought to the ſea, it muſt have remained diſſolved, and have been diffuſed through it in ſuch a manner as, from the ſmall quantity of it apparently exiſting in nature, not to be capable of being diſcovered; and no cauſe can be imagined, in conformity with that ſyſtem, by which it could be precipitated. Even the theory which is employed for accounting for the production of ſea ſalt, extravagant as it is, cannot be applied in the preſent caſe, for, if the ſea water, holding the carbonate of ſoda in ſolution, had been expoſed to a local heat capable of converting it into vapour, and of thus precipitating what was diſſolved in it, it is obvious that pure carbonate of ſoda could not have been depoſited, but muſt have been mixed with muriate of ſoda, and

every other ſubſtance which the ſea water had held in ſolution; or rather, if it ever had been brought to the ſea, it muſt have immediately been decompoſed by the muriate of magneſia preſent, and could never have exiſted in the waters of the ocean. The Huttonian theory is therefore actually incapable of affording a ſuppoſition, by which carbonate of ſoda, as a foſſil ſubſtance, could be formed. Mr. Kirwan adds, as a proof that it has not been fuſed, its containing ſome grains of ſand, which would neceſſarily, in ſuch a caſe, have been vitrified. And were it ſuppoſed to have been in fuſion, it would require to be explained how the preſſure was ſo nicely adjuſted as to admit of the greater part of its water of cryſtallization being driven off, while none of its carbonic acid, even though it be ſuperſaturated with it, and of courſe retains the exceſs by a very weak affinity, had been expelled.

The production of this ſalt, if it really be part of a vein, may be explained on the Neptunian ſyſtem by a ſimilar hypotheſis to that propoſed; to account for the formation of rock ſalt, and from its haſty cryſtallization, or its ſuper-ſaturation with carbonic acid, (which, from its analyſis, is found to be the caſe,) the diminiſhed quantity of water of cryſtallization in it may be accounted for. If the fact, however, be true, which Bergman ſtates, that the carbonate of ſo-

da found in the earth, both in India and Africa, is free from common ſalt at the ſurface, but becomes contaminated by it as it deſcends deeper, it is probable that this ſalt may have been formed at the ſurface from the decompoſition of muriate of ſoda by ſome unknown power, and from this mode of formation its containing leſs water of cryſtallization might ariſe.

In concluding this argument with regard to the ſaline ſubſtances, the formation of one of them which is not noticed in the Huttonian geology, gypſum or ſulphat of lime may be ſtated. This ſalt is ſoluble in 500 times its weight of water; of courſe, had it exiſted in the ancient world, and been brought by diſintegration to the ocean, it muſt have remained diſſolved in the water till that fluid was ſaturated with it; and there probably does not exiſt in nature a quantity ſufficient for that purpoſe. It would then remain to be explained how it could be collected in particular places, and be precipitated. The theory, applied to the formation of ſea ſalt, will here be of no avail; for ſhould we ſuppoſe it preſent in a large proportion in ſea water in a particular place, the evaporation of that water could not account for its conſolidation, ſince it muſt have been accompanied with ſea ſalt, a ſubſtance of which, according to the experiments of Sauſſure, gypſum frequently does not

contain an atom. This, therefore, is another foſſil, for the formation of which the Huttonian theory cannot account. Its origin, on Neptonian principles, may probably be explained in the ſame manner as that of rock ſalt.

The ſtate in which the METALS are found in nature, either pure or combined with other ſubſtances, appears to afford a ſtrong argument in favour of the Huttonian hypotheſis; for the fluidity from which they have conſolidated may have been produced by fuſion, but we ſcarcely can point out by what ſolvent it could have been effected. Gold, ſilver, copper, and ſome others, are frequently found native or uncombined. "Of all ſuch ſpecimens it may be ſafe-"ly affirmed, that if they have ever been fluid, "or even ſoft, they muſt have been ſo by the "action of heat; for to ſuppoſe that a metal has "been precipitated pure and uncombined from "any menſtruum is to treſpaſs againſt all analo-"gy, and to maintain a phyſical impoſſibility*."

The aſſertion in this paragraph is much ſtronger than what the facts can eſtabliſh; for although it may be difficult to point out the mode in which metals have actually been precipitated, yet their precipitation from any menſtruum, ſo far from being a phyſical impoſſibility, is what happens

* Illuſtrations, &c. p. 59.

every day; and can be effected at pleaſure. Metals in a ſtate of combination with acids, and in ſolution in water, are thrown down in their metallic ſtate by each other, by hydrogen, ſulphurated hydrogen, and various other inflammable bodies. The Neptunian will readily acknowledge, that it is extremely difficult to point out, even by hypotheſis, by what particular agency the metals found in nature had been diſſolved and precipitated, but at the ſame time he has ample demonſtration, that it is in this way, and not by fuſion, that they have been formed.

This proof is obtained from the cryſtallized ſtate in which they are frequently found. "Specimens of quartz, containing gold and ſilver "ſhooting through them, with the moſt beauti-"ful and varied ramifications, are every where "to be met with in the cabinets of the curious, "and contain in their ſtructure the cleareſt proof "that the metal and the quartz have been both "ſoft, and have cryſtallized together *."

It may be pronounced a phyſical impoſſibility, that from ſimple fuſion quartz and gold, or quartz and ſilver, could cryſtallize together ſo as to exhibit theſe appearances. Theſe metals are fuſed at a heat, which, compared even with

* Illuſtrations, &c. p. 59.

the degrees of heat we have it in our power to produce, may be termed very moderate, while quartz we are unable to fuſe. If, therefore, both ſubſtances were in fuſion, on a reduction of temperature, the quartz muſt have become ſolid long before the metal; and it is abſolutely impoſſible that the metal could have ſhot through the quartz. Or, to place this in a point of view more preciſe, gold melts at a temperature equal to 32 of Wedgewood's pyrometer, and at all temperatures above this it muſt remain fluid: Quartz does not melt at the higheſt heat that has been accurately meaſured; but, according to the experiments of Sauſſure, it is not leſs than 4043 of Wedgewood's ſcale, and conſequently at every temperature below this, muſt continue ſolid.— Grant, therefore, to the Huttonian, that both quartz and gold were in fuſion, it is evident that on a reduction of temperature to 4000, the quartz would become ſolid, or cryſtallize; but it is equally certain, that at this temperature, and for more than 3900 lower, the gold muſt remain fluid: the ſuppoſition, therefore, that the gold could become ſolid, and ſhoot through the fluid quartz, involves a direct contradiction in terms, or ſuppoſes a phyſical impoſſibility; and of conſequence, the various appearances in theſe ſpecimens which prove that the metal and quartz had cryſtallized together, or that the former had

cryſtallized firſt, prove, as much as any phenomena can, that theſe cryſtallizations could not be from ſimple fuſion. It will not ſurely be preſumption to affirm, that if this be not admitted as an undeniable concluſion, as affording even a demonſtration as certain as any can be, all reaſoning on the ſubject muſt be given up, for it is impoſſible to conceive a propoſition more evident, or the reverſe of which involves a more palpable contradiction in terms. The ſtatement of theſe facts, as favourable to the Huttonian ſyſtem, affords a ſtriking example how far the mind may be miſled by a favourite hypotheſis, the very appearances which prove its falſity being adduced as proofs of its truth.

One mode might perhaps occur in which it might be attempted to remove this difficulty. It may be conceived, that the quartz had firſt become ſolid, and that merely the fiſſures of it had been filled with cryſtallized gold. But ſuch a ſuppoſition has been very clearly refuted by Mr. Playfair himſelf. "Between the channels "in which the metal pervades the quartz, and "the ordinary cracks or fiſſures in ſtones, there is "no reſemblance whatever; a ſyſtem of hollow "tubes winding through a ſtone (as the tubes "in queſtion muſt have been before they were "filled by the metal), is itſelf far more incon-"ceivable than the thing which it is intended

" to explain; and laſtly, if the ſtone was per-
" forated by ſuch tubes, it would ſtill be infi-
" nite to one that they did not all exactly join,
" or inoſculate with one another *."

The concluſion which may be drawn from this argument is ſomewhat ſingular: The ſtronger the objection is ſtated, and it cannot be more ſtrongly urged than it has been by Profeſſor Playfair, the more favourable on the whole is it to the Neptunian ſyſtem. Let it be placed in the cleareſt light, ſuppoſe it even to be ſo ſtrongly urged, that the Neptuniſt is unable to give any probable conjecture as to the mode in which the metals have been formed by ſolution; what is the fair concluſion? It is, that difficulties of this kind are inſeparable from the ſubject, or rather from our imperfect knowledge, and are therefore comparatively of little importance, if they do not involve inconſiſtencies with the principles of the theory, or with eſtabliſhed facts. From the appearances of the metals, from the diſſemination of cryſtallized gold in quartz alone, we have a clear and unexceptionable demonſtration that they do not owe their origin to fuſion. No other mean can be pointed out, or even imagined, but ſolution; it has already been pointed out, how from ſolution ſimultane-

* Illuſtrations, &c. p. 245.

ous conſolidation might take place; and it is ſufficiently obvious, that though quartz is leſs fuſible than gold, and muſt therefore conſolidate before it from *fuſion*, it might be more ſoluble in the menſtruum in which both were diſſolved, and might therefore remain fluid while the gold cryſtallized from *ſolution*. But were it even impoſſible to conjecture how they ſhould have conſolidated from ſolution, ſo as to produce the appearances obſerved, the concluſion muſt ſtill be drawn that they had been formed in this mode; for on the one hypotheſis we ſhould have only a deficiency of explanation, in the other, a direct and poſitive contradiction to an eſtabliſhed truth.

This proof of the aqueous origin of metals is therefore capable of being carried a great length, ſince there are certainly no foſſils which at firſt view would appear leſs likely to have been formed by water. It alſo of itſelf eſtabliſhes a ſimilar origin to almoſt every other mineral, for the metals are ſo intimately connected with ſo many of them, with quartz, carbonate, and fluate of lime, ſulphate of barytes, and many others, that whatever has been the origin of the one, muſt have been the origin of the other.

There are ſome other facts reſpecting the native metals, cited as proofs of the Huttonian theory, particularly the large ſpecimens of iron

found native in Sibetia and Peru. The moſt remarkable circumſtance with regard to theſe maſſes is their largeneſs. That found in Siberia weighs 15 tons, and is ſoft and malleable. The American ſpecimen is alſo very large, and there is a peculiar appearance connected with it,—the impreſſions of the feet of men and birds on its ſurface. Theſe maſſes are concluded to have been formed by fuſion, and to have been part of the contents of a vein waſted away, from which, the iron being the more durable ſubſtance has been left on the ſurface of the ground.

In conſidering the ſingular circumſtances attending theſe maſſes, what would one conclude who was guided by the common rules of reaſoning. He would infer merely that their origin was at preſent inexplicable. The defender of the Huttonian theory purſues a different mode; becauſe it is inconceivable, as he imagines, that they could have been formed by precipitation from ſolution, he therefore concludes, that they had been formed by fire, and brings them forward as proofs of his ſyſtem. With regard to the very peculiar circumſtance attending one of theſe maſſes we receive no information; we are not told how birds and men found their way to the central regions, and left the impreſſions of their feet on the fuſed iron,

but the obſervation is merely made, that ſuch circumſtances " are not to be accounted for on " any hypotheſis, and certainly require more " careful inveſtigation *." The concluſion would have been more accurate that they demonſtrate that theſe maſſes could not have been formed by fuſion and injection. The fact might be ſtated as a ſingular one in a ſyſtem of natural hiſtory, but in reaſoning on a theory of the earth, it ought while involved in ſuch obſcurity to have found no place, far leſs ſhould it ever have been brought forward as a proof of the fuſion of minerals.

The native combination of metals with ſulphur, affords another argument to the Huttonian geologiſt. It is obſerved, that neither the metal nor the ſulphur is ſoluble in water; and that the metallic ſulphuret, even when formed, is decompoſed by water, while, on the other hand, ſulphur, and the greater number of the metals, can be fuſed and combined by heat. It is hence concluded, that pyrites, and other native combinations of ſulphur with metals, muſt have been formed by fuſion, and this concluſion has been employed as an argument to prove the igneous origin of a number of foſſils in which theſe ſulphurets are found.

* Illuſtrations, &c. p. 240.

The argument, *a priori*, that pyrites cannot be formed in the humid way, becauſe neither the ſulphur nor the metal is ſoluble in water, is founded on an erroneous concluſion; for, granting the fact, each might have been combined with other ſubſtances which would render them ſoluble, and, in this ſtate of ſolution, they might, from the chemical affinity ſubſiſting between them, leave the ſubſtances with which they were united, and combine together. This actually happens in the example of ſulphurated hydrogen, with a number of the metallic ſalts or oxyds. If this compound of hydrogen and ſulphur, be introduced into a ſolution of any of the ſalts of lead, the hydrogen combines with the oxygen of the metallic oxyds, and the lead combines with the ſulphur, forming a compound which, according to the obſervation of Vauquelin, has all the properties of galena. It is not even neceſſary that the metal ſhould be combined with an acid, for if humidity be preſent it will be oxydated, and upon this oxyd the ſulphurated hydrogen is capable of acting and producing a metallic compound, or the ſame ſubſtance is even capable of acting on the pure metals.

This argument muſt be admitted as perfectly concluſive, when it is proved, that pyrites,

and other compounds of the metals with ſulphur, are formed by nature in the humid way, and of this the proofs are abundant.

Thus pyrites is found in ſituations which clearly indicate its aqueous formation. It is aſſociated with calcareous cryſtals lining the internal cavity of ſhells, which could not have been in fuſion, ſince the heat neceſſary to fuſe the pyrites, or the calcareous cryſtals, muſt have deſtroyed the texture of the ſhell,—it is often to be traced in the impreſſions of organic ſubſtances, particularly of animal remains in coal and other foſſils; it exiſts in bituminated wood, which, it muſt be granted, could not have been fuſed; and it has been found forming on the ſurface of wood, in mines, and in other ſituations, which eſtabliſh the ſame concluſion, of which ſeveral examples are ſtated by Mr. Kirwan *.

The obſervations of Mr. Wiſeman on the effects of the waters of the Mere of Diſs on metallic ſubſtances; and the farther experiments and obſervations of Mr. Hatchet, on the ſame ſubject, are alſo particularly valuable in proving the humid formation of theſe compounds. Mr. Wiſeman obſerved, that flints, and other ſtones, immerſed for ſome time in this ſtagnant water,

* Geological Eſſays, p 400

were incruſted with a metallic ſtain. This, by analyſis, was found to be ſulphuret of iron; and when copper was kept in this water, it was encruſted by a ſubſtance which was found to be compoſed of 70 parts of copper, 16.6 of ſulphur, and 13.3 of iron. This encruſtation was even found in a cryſtallized ſtate. Theſe experiments on this ſubſtance, and on the encruſtation of martial pyrites on the flints, were confirmed by Mr. Hatchet. With reſpect to the latter, he obſerves, there could be no heſitation; and the former he conſidered as "in every property "ſimilar to that rare ſpecies of copper ore, call-"ed by the Germans *Kupfer ſchwärtze*, (cu-"prum ochraceum nigrum) and abſolutely the "ſame." At the deſire of Mr. Hatchet, ſilver was immerſed in this water, and it was found to be encruſted with a ſubſtance "ſimilar in every "reſpect to the ſulphurated or vitreous ore of "ſilver, called by the Germans, Glaſertz." The ſame diſtinguiſhed chemiſt adds, that effects ſimilar to theſe, on a larger ſcale, there is reaſon to believe, "have been, and are now daily pro-"duced in many places. The pyrites in coal "mines have probably, in great meaſure, thus "originated. The pyritical wood may thus "have been produced; and by the ſubſequent "loſs of ſulphur, and orydation of the iron, this

"pyritical wood appears to have formed the "wood like iron ore, which is found in many "parts *." Of the formation of metallic ſulphurets in the humid way, there can therefore be no doubt, and inſtead of having any difficulty to encounter in explaining the origin of theſe ſubſtances, the Neptuniſt may juſtly bring them forward, as affording a proof of his theory, and a proof of conſiderable importance, from the extenſive connections they have with other foſſils.

If it were neceſſary to ſay more on this ſubject, which perhaps it is not, it might be remarked, that from the manner in which theſe compounds are aſſociated with certain foſſils, we have the ſame demonſtration as in the example of the pure metals, that they cannot have been formed by fuſion. Not only the ſulphuret of iron, but thoſe of antimony, mercury, and ſilver, are frequently found cryſtallized or diſſeminated through quartz and other foſſils. Now theſe ſulphurets are very eaſily fuſed, while theſe foſſils are comparatively infuſible; it is therefore impoſſible that the former could have cryſtallized within the latter, or been diſſeminated through them, ſince, to admit of its regular cryſtallization, or even of its diſſemination in another

* Philoſophical Tranſ. 1796. p. 567.

body, that body muſt have afforded little or no reſiſtance, or been entirely fluid, or extremely ſoft; but quartz could not be fluid from fuſion, and at the ſame time ſulphuret of antimony or mercury have been ſolid.

The ſtructure and appearances of GRANITE as a foſſil, have been brought forward by Dr. Hutton as favourable to his hypotheſis. There can be no doubt that this rock has at one time been fluid. Its conſtituent parts, particularly the felſpar, and ſometimes the quartz, are chryſtallized, and it not unfrequently contains other cryſtallized foſſils. This fluidity from which it has been conſolidated, Dr. Hutton conceives is proved to have been that of fuſion from the parts of the granite impreſſing each other. The ſpecies of granite termed Graphic, is ſtated, particularly as eſtabliſhing this deduction. In it the felſpar is cryſtallized in its uſual rhomboidal form, and theſe cryſtals impreſs the quartz, put it aſide as it were, and give it its particular ſituation along the ſides of the rhomboidal felſpar. "Hence "this granite is not a congeries of parts, which, "after being ſeparately formed, were ſomehow "brought together and agglutinated, but it is "certain that the quartz at leaſt was fluid when "it was moulded on the felſpar." And "this "fluidity was not the effect of ſolution in a "menſtruum, for in that caſe one kind of cry-

" ſtal ought not to impreſs another, but each " of them ſhould have its own peculiar ſhape*."

It has already been ſhown, how ſimultaneous conſolidation may take place from ſolution; and the ſtructure of granite can, from this circumſtance, furniſh no argument againſt its aqueous origin. But it affords the cleareſt demonſtration, that it has not been formed by fuſion. Felſpar is a ſubſtance incomparably more fuſible than quartz, the one varying from 120 to 150, the other being 4043 of Wedgewoods ſcale. It is a propoſition, therefore, ſelf evident and undeniable, that in the ſame maſs quartz could not be fluid when felſpar was ſolid; and therefore, ſince in this graphic granite the quartz is moulded on the cryſtallized felſpar, (and, in the greater number of granites, the felſpar is cryſtallized while the quartz is not) the fluidity whence both have been conſolidated cannot have been fuſion from heat.

The force of this argument, from the cryſtallization of felſpar in granite, may be eſtimated by a very ſimple conſideration. If the appearance of this foſſil had been the reverſe of what it is, if the quartz had cryſtallized and ſhot through the felſpar, would it not have been brought forward as a proof, or at leaſt as a ſtrong

* Illuſtrations, &c. p. 86.

preſumption, that both had conſolidated from fuſion? Yet the fact, as it is, is ſtill ſtronger in proving, that ſuch could not have been their origin, ſince it is an evident impoſſibility, that, in two ſubſtances fuſed together, the moſt fuſible ſhould concrete before the one that was leaſt fuſible, or required the greateſt heat to keep it fluid. Had the former even been congealed by ſome cauſe, of which we can form no conception, the high temperature keeping the other, fluid, muſt have again immediately fuſed it.

Under this head may be noticed ſeveral facts, affording an argument of a ſimilar kind;—thoſe in which foſſils are regularly cryſtallized, in others which are much leſs fuſible. Thus beſides the cryſtallization of felſpar in granite, regular cryſtals of it are not unfrequently found embedded in quartz, a proof which cannot be eluded, that the felſpar has become ſolid, while the quartz remained fluid, contrary to what muſt have happened from the known fuſibilities of theſe ſubſtances, if they had conſolidated from fuſion. Shorl is a ſubſtance of comparatively eaſy fuſibility, yet it is often cryſtallized in quartz, the fibres of the ſhorl being finer even than the human hair, ſhooting through a large maſs of quartz in every direction, and with various wavings and incurvations, ſo as to prove deciſively that the quartz had been completely

liquid when the ſhorl cryſtallized. Aſbeſtos is another ſubſtance found ſhooting in the moſt delicate fibres through quartz, though it melts at 378 of Wedgewood's ſcale. And, without enumerating more examples, micaceous ſhiſtus is a rock of very difficult fuſion, yet it is the common matrix in which garnets are enveloped, and though theſe are much more fuſible, they are cryſtallized in its ſubſtance with the greateſt regularity. It muſt be ſuperfluous to repeat the argument from ſuch facts. If they do not prove that theſe foſſils have not been formed by fuſion, no concluſion can be eſtabliſhed in geology, and we may relinquiſh every attempt to theoriſe.

WHIN or TRAP is a rock into which granite inſenſibly graduates, ſo that what is proved with reſpect to the one, may be conſidered nearly as proved with regard to the other. There are ſome particular facts, however, with regard to whin, which are ſuppoſed by the defenders of the Huttonian theory to prove its igneous origin.

Whin, it is ſaid, reſembles lava in its appearance ſo much, that ſome varieties of it have been miſtaken for volcanic products. This reſemblance "leads to ſuſpect, that the two ſtones "have the ſame origin, and that as lava is cer-"tainly a production of fire, ſo probably is

" whinſtone *." Any diverſity exiſting between them conſiſts principally in whinſtone containing calcareous ſpar, which lavas do not; and this diverſity is likewiſe explained in the Huttonian ſyſtem; as this ſubſtance might be formed in whin, from the fuſion of it having been under an immenſe preſſure, while lava, being in fuſion expoſed to the air, it muſt have been decompoſed. " Thus," it is added, " whinſtone is to be " accounted a ſubterraneous or *unerupted* lava; " and our theory has the advantage of explain- " ing both the affinity and the difference be- " tween theſe ſtony bodies, without the intro- " duction of any new hypotheſis. In the Nep- " tunian ſyſtem, the affinity of whinſtone and " lava is a paradox which admits of no ſolu- " tion †."

To this argument it may be replied, that the mere reſemblance in appearance between different foſſils is a very weak proof of a ſimilar origin, ſince in many caſes cloſe reſemblances are to be traced between foſſils altogether different in their nature. But were it a juſt concluſion in general, in the preſent caſe it is not ſo, becauſe the reſemblance can be otherwiſe very eaſily explained. It is extremely probable that lava

* Illuſtrations, &c. p. 68.
† Illuſtrations, &c. p. 69.

is formed from rocks of the nature of whin, fuſed by the volcanic fire. It is apparent, from the deſcriptions of Spallanzani, and other mineralogiſts, that rocks of this ſpecies are the baſis of volcanic countries: zeolite, leucite, and other foſſils, uſually found in rocks of this order, are likewiſe contained in lavas, altered, but, according to the opinion of the beſt mineralogiſts, not formed by the volcanic fire. And, laſtly, the excellent analyſes of Dr. Kennedy prove the near reſemblance in compoſition of trap and lava. They ſhow, as he himſelf obſerves, "that whins, and a certain claſs of lavas taken "from remote quarters of the globe, conſiſt of "the ſame component elements united in each, "nearly in the ſame proportion. The only cir-"cumſtance in which they materially differ, is "the loſs of ſome volatile matter in the fire, "which is peculiar to the whins alone." The concluſion, therefore, evidently amounts even to more than a high probability that lava has been formed from the fuſion of trap; and if this be true, the reſemblance between them is no proof whatever of their having had a ſimilar origin. Whatever may have been the origin of the trap,—although it be of aqueous formation, it is perfectly conceivable, that if fuſed, as none of its principles are loſt, it might form a ſub-

ſtance very ſimilar in its properties to the trap in its original ſtate. It is equally evident, that the difference between trap and lava, that of the latter containing no carbonate of lime, is on this ſuppoſition fully explained, ſince, if the trap were fuſed in an open volcanic fire, the carbonic acid would be expelled from the lime. The affinity, therefore, between lava and whinſtone, in ſome points, and their difference in others, are fully accounted for, and cannot be regarded as forming, "in the Neptunian ſyſtem, a paradox which admits of no ſolution."

But there is a difference between theſe ſubſtances not noticed by the Huttonian geologiſt, and for which he will find it difficult to account. We are told that trap differs in lava, in nothing but in the circumſtances of their formation, the one having been melted matter erupted at the ſurface while fluid, the other having been thrown up among ſolid ſtrata, and conſolidated under an immenſe preſſure. Hence is explained the preſence of carbonate of lime in the one, while it is not found in the other; and it follows, from the opinion itſelf, that lava and trap ſhould differ from each other in nothing but in ſuch properties or appearances as are capable of being produced by the cauſe thus ſpecified. Will the Huttonian geologiſt then inform us why agates,

and maſſes or veins of quartz, or even regular cryſtals of it, which are abundant in baſalt, are not to be found in lava? The abſence of compreſſion could not prevent their formation, or render them more ready to be deſtroyed if they were formed; and it is evident that lava and trap ought, according to the Huttonian theory, to differ in nothing but what this abſence of compreſſion in the one caſe could occaſion.

The columnar ſtructure which the cloſe-grained whin ſometimes aſſumes, forming baſalt, has been ſuppoſed a proof of its igneous origin, becauſe the ſame ſtructure, it is ſaid, is ſometimes aſſumed by the lava actually erupted from volcanos. It is to be obſerved, that in the greater number of lavas, cooled under every variety of ſituation, either ſlowly by expoſure to the air, or rapidly by having flowed into the ſea, this columnar appearance is not to be obſerved; and many of the inſtances which have been given are extremely doubtful, from rocks not volcanic having been ſo often confounded with lavas. If it be admitted that real lavas do ſometimes aſſume a columnar form, (and ſuch inſtances, if they exiſt, are extremely rare,) ſtill theſe facts clearly ſhow that this has not been an effect ariſing from their ſpecies of fluidity, or their mode of conſolidation, as in by far the greater number of caſes in which theſe cauſes muſt have

equally operated, it is abſent. Did it ariſe, indeed, from either of them, it ought to be obſerved in almoſt all the varieties of whin, and might even be expected in the other unſtratified rocks. It may therefore be aſcribed with more probability to another cauſe;—the peculiar nature or compoſition of the matter of which lava conſiſts: And if this, as there is every reaſon to believe, be the real cauſe, it is evident, that ſince baſalt is perfectly ſimilar in compoſition to lava, it might aſſume the ſame form in its conſolidation from an aqueous origin: that, in ſhort, if the form do not originate from the mode of conſolidation, but from a property belonging to the ſubſtance itſelf, and originating in its compoſition, it may equally be exhibited by lava becoming ſolid from fuſion, and baſalt becoming ſolid from ſolution. It is not improbable that this property may ariſe chiefly from the predominance of argil in theſe foſſils, as it is found to take place even in other argillaceous foſſils, as in the argillaceous iron ore, ſome varieties of marl, and even ſome of argillaceous ſandſtone, —ſubſtances, ſome at leaſt of which have evidently never been fuſed.

" A mark of fuſion, or at leaſt of the opera-
' tion of heat, which whinſtone poſſeſſes in com-
" mon with many other minerals, is its being
" penetrated by pyrites—a ſubſtance, as has been

already remarked, that is of all others moſt excluſively the production of fire *." The ſuperior probability of pyrites being of watery origin has been already ſhown, and of courſe this fact becomes a ſtrong argument for the aqueous formation of whin. A ſimilar argument is that derived from the preſence of agates in trap, theſe foſſils being ſuppoſed to give indications of having been formed by fire. This ſuppoſition has already been conſidered, and the concluſion endeavoured to be proved, that, in common with other foſſils, they owe their formation to the agency of water.

The laſt argument for the igneous origin of whin, which has not been noticed, is that deduced from the experiments of Sir James Hall. It had been often ſtated, as an objection to the opinion that baſalt was of igneous origin, that it ought to have a vitreous luſtre and fracture, ſince from melting any earthy combination ſome ſpecies of glaſs is always produced, and ſince baſalt itſelf by fuſion actually forms a real glaſs. Sir James Hall, by a number of excellent experiments, has clearly ſhown, that when this ſtone is brought into fuſion, if the refrigeration of it be very ſlow, it aſſumes the ſtony character, or is ſcarcely diſtinguiſhable

* Illuſtrations, &c.

from the real baſalt, and that it is only when haſtily cooled that it acquires any of the properties of glaſs.

By ſome of the defenders of the Huttonian theory the concluſions from theſe experiments have been carried much farther than they warrant, or than their author has ſtated. Becauſe from the fuſion of trap or baſalt a ſubſtance ſimilar to theſe foſſils was by particular management produced, it has been concluded that this affords a proof of fuſion being the means of their firſt formation. This concluſion is an evident miſtake: the fuſion of the baſalt, as it contained no volatile ſubſtance of importance, could not alter its compoſition, and of courſe when it again became ſolid, it would ſtill be endowed with its former properties. Even if the ſimple earths of which baſalt conſiſts had been mixed together in the proportions which its analyſis affords—if by fuſion theſe had been brought into union, and formed a ſubſtance ſimilar to natural baſalt, ſtill this would not have proved the igneous formation of this foſſil; for as all compounds derive their properties from their compoſition, if their conſtituent parts are capable of being brought into union by the humid way, and alſo by fuſion, it may be expected that a ſimilar compound will in both caſes be formed; and the actual production of ſuch a compound

by one of theſe modes, would not prove that it was incapable of being formed in the other. Had therefore this experiment been made, no concluſion of this kind could have followed, ſtill leſs can it be inferred from merely fuſing natural baſalt, and finding that by ſlow cooling a ſubſtance ſimilar to it is formed. In ſtrict reaſoning, this experiment adds nothing poſitive to the evidence of the Huttonian ſyſtem; it only removes an objection which could have been urged with juſtice againſt it,—and in this point of view it may be prized by the defender of that doctrine.

It is remarked, that "the experiments of another ingenious chemiſt, Dr. Kennedy, have "ſhown, that whinſtone contains mineral alkali, "by which of courſe its fuſion muſt have been "aſſiſted*." It may be added, that the preſence of this alkali would not leſs contribute to the ſolubility of the matter of whin in water.

Beſides the proofs of the aqueous origin of whin, which have been noticed in the courſe of this argument, it may be added, that there are facts which indiſputably eſtabliſh it. Theſe are, the exiſtence of foreign bodies in it, which could not poſſibly have been preſent had it been thrown up in a ſtate of fuſion from the

* Illuſtrations, &c. p. 80.

bowels of the earth. Mr. Jamieſon has given a very full enumeration of theſe, on authorities which it would be preſumption to diſpute. Thus, Werner found in wacken, great trees with branches, leaves, and fruit, and obſerves, that it is ſometimes found to contain deers horns. Sauſſure obſerved in it bones of quadrupeds; and other mineralogiſts have found ſhells, vegetable impreſſions, and fragments of wood. It has been ſtated with reſpect to ſome of theſe, that they are found, not in the baſalt, but in ſtrata which alternate with it; and in ſome caſes this appears to have been the caſe. But in the obſervations of Werner and Sauſſure we are not at liberty to ſuppoſe that they committed ſo obvious a miſtake; and therefore theſe obſervations are deciſive proofs of the aqueous origin of theſe rocks.

The properties therefore of the ſubſtances compoſing the unſtratified rocks equally with their poſitions exclude the operation of fire, and prove them to be of aqueous origin.

We have thus completed the examination of the HUTTONIAN and NEPTUNIAN theories; and it cannot be difficult to form an opinion on their merits. To the Huttonian ſyſtem belongs the praiſe of novelty, boldneſs of conception, and extent of views. Its author has aſpired not merely to ac-

count for the preſent appearances of the earth, but to trace a ſyſtem in which the formation of ſucceſſive worlds is developed; he has ſought to extend that order and arrangement, that principle of balance and reſtoration obſerved in all the departments of nature, to the conſtitution of the globe itſelf; and he has ſucceeded in drawing an outline which gratifies the imagination with the ſemblance of grandeur and deſign.

But theſe are the only merits of the theory, and they have certainly been much over-rated by the partiality of its defenders. When full ſcope is given to the imagination, when the reſtraint of ſtrict induction is not impoſed, experience, and eſpecially experience in geology, has ſhown, that it is no difficult taſk to conſtruct a ſyſtem, and to give it in appearance that unity of principle, and adaptation of parts which are the attributes of a perfect theory. This may be what the author of the Huttonian ſyſtem has attained; but a more juſt and cautious reaſoner would have ſtartled at the *poſtulata* the ſyſtem requires, and would not have thought their extravagance and improbability, their inconſiſtency with phenomena, and their oppoſition to eſtabliſhed truths, compenſated by the greatneſs or novelty of its views.

It can ſcarcely be neceſſary to juſtify theſe obſervations by any recapitulation of the evi-

dence of this theory. In appealing to the proof from induction, we have found the phenomena of geology entirely at variance with its principles. It has not been neceſſary to ſearch for minute deficiences, or ſelect a few concluſive arguments from a number that are doubtful or obſcure; the whole ſeries is clear and convincing; the poſitions and relations of the great maſſes of the globe, and the properties and appearances of individual foſſils, being altogether incompatible with the ſuppoſition of their having been formed by a central fire. Its firſt principles we have found not merely in the higheſt degree improbable, but abſurd and phyſically impoſſible. It aſſumes the exiſtence of an intenſe heat in the interior parts of the earth, without aſſigning any cauſe by which it could have been produced; it ſuppoſes an exertion of that heat, not merely at the formation, or during the period of the exiſtence of a world, but for a time abſolutely unlimited; and it advances theſe ſuppoſitions in expreſs contradiction to the known and eſtabliſhed laws of the power it employs. Such characters bring the Huttonian hypotheſis under the ſame claſs with thoſe geological ſyſtems which have preceded it,—ſyſtems which have been the meteors of their day, and have ſunk into obſcurity; and which, to uſe the language of Dolomieu, will never be mentioned

in the hiſtory of the ſcience, but as pointing out the by-paths in which thoſe may wander who devote themſelves to the contemplation of nature.

With this ſyſtem, the Neptunian theory forms a complete contraſt. It preſumes not to carry its reſearches paſt the commencement of the preſent world, or to extend them beyond its termination; it is ſatisfied with endeavouring to trace the cauſes of the appearances which at preſent exiſt; and the characters of its explanations are theſe of fair and legitimate deduction. All the phenomena of geology conſpire to prove that water has been the great agent by which minerals have been formed, and the ſurface of the earth arranged. While the ſcience remains in an imperfect ſtate, deficiencies muſt be found in the application of this principle which induction eſtabliſhes. Such there may be in the Neptunian theory; and that there ſhould, is even a preſumption of its truth. But we diſcover no inconſiſtencies with that principle, nor contradictions to known truths. We find in it, in reality, what we ſhould at preſent expect in a juſt theory of the earth: not the magnificent pretenſions of an artificial ſyſtem,—not the ſplendid illuſions created by a bold imagination, but a ſeries of inductions more or leſs perfect, referred to a common principle, and occaſional-

ly connected by a moderate and rational hypotheſis. In a word, it may be conſidered as the commencement of a ſyſtem which poſſeſſes the ſtability of truth and which time will extend and improve.

In this Comparative View the author has endeavoured to conduct the diſcuſſion with fairneſs and candour: he is not conſcious of having ſuppreſſed a ſingle argument of importance, or of having ſtated any ſo as to diſguiſe its ſtrength: And he truſts, that, in ſupporting the ſyſtem he defends, he has not been wanting in that reſpect ſo juſtly due to the diſtinguiſhed author of the "Illuſtrations of the Huttonian "Theory."

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